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American Anthropologist

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FURTHER CONSIDERATION OF THE OCCURRENCE OF HUMAN REMAINS IN THE PLEISTOCENE DEPOSITS AT VERO, FLORIDA

BY OLIVER P. HAY

IT is here taken for granted that anybody who is sufficiently interested to read this article has either already studied the papers written on the subject by Sellards, MacCurdy, Vaughan, Hrdlička, R. T. Chamberlin, and Hay, or that he will proceed to do so. This course will obviate the necessity of redescribing the topographical, geological, and paleontological situation at Vero.

After a study of the problems presented at Vero, the authors just named each expressed his independently formed opinion in a symposium which appeared in the January (1917) number of the *Journal of Geology*. Since that time Doctors Sellards and MacCurdy have contributed interesting articles on the subject[1] and the writer has published a condensed statement of his views.[2] It is to be regretted that Dr. Hrdlička has not, up to the time of writing these lines, been able to present his projected detailed report[3] in which were to be discussed the broader anthropological and archaeological problems concerned in the matter.

At any rate, it seems to the writer that progress is being made;

[1] *American Anthropologist* (N. S.), vol. 19 (1917), pp. 239–261.

[2] *Journal Washington Academy of Sciences*, vol. VII, pp. 358–360.

[3] *Symposium*, p. 50.

that some questions are already settled and others well on the way. The writers are agreed that there are presented at Vero three distinct geological deposits of Quaternary age; that in all of these there are found remains of animals which have hitherto been regarded as belonging exclusively to the Pleistocene; that human bones and artifacts occur in the middle and the uppermost of the three strata in some sort of association with remains of these Pleistocene vertebrates, and that the human bones and the artifacts differ little, if at all, from those of pre-Columbian Indians.

It appears further that the following questions are still matters of contention:

1. Are the vertebrate fossils found in deposits no. 2 and no. 3 there by primary inclusion, or have they been washed in from older deposits?
2. Were the human bones introduced into the two deposits by artificial burial or did they reach their recent positions by natural means?
3. Have any of the human remains found in no. 2, the age of the deposit itself?
4. What is the geologic age of stratum no. 2?
5. What is the geologic age of stratum no. 3?

1. Dr. R. T. Chamberlin[1] has taken the position that the vertebrate fossils in no. 2 and no. 3 have been washed in from older beds and within comparatively recent times. Not far back from the valley where these deposits occur, he found a stratum from 2 to 4 feet thick consisting of a "dark-brown to true-black, firmly indurated sand, or sandstone, cemented by ferric hydroxide and organic matter." Above this there is a soft, spongy, peaty layer, the remains of an ancient bog, which varies in thickness from nothing to 6 inches. These old bog materials are covered up by wind-blown sands to a depth ranging from 7 feet on the eastern border to hardly 2 feet one mile further west. Chamberlin has expressed the opinion that it is this "Pleistocene bog accumulation and its associated deposits which originally housed the old mammalian bones."

It seems to the writer that Sellards's last paper[2] disposes of this theory. The main canal and its two laterals gash in three

[1] *Symposium*, p. 36.

[2] *American Anthropologist*, vol. 19, pp. 239–251.

different directions the deposits noted by Chamberlin; and the banks and dumps of these canals and of the old drainage channels have been thoroughly searched and no fossils have been found in that old bog. This absence of fossils is remarkable because some bones and teeth might well have been expected to occur there. In connection with this dearth of bones in the bog, must be taken into consideration the fact that only those fossils, if any had been present, which had been eroded out of the bog materials along the banks of those little rivulets could have been swept down into the deposits of no. 2 and no. 3; for the remainder of the area has always been protected by the stratum of wind-borne sands.

Chamberlin has found in the deposits just mentioned what he regards as portions of the indurated sandstone of the old bog which have been carried down by the streams and so rolled and eroded that some of them are now designated as "cannon-balls." Now, if those vertebrate fossils had originally been buried in the peat of the bog they would have been at the best rather soft and brittle. What chance then would they have had for arrival at their destination in any recognizable condition when blocks of indurated sandstone were rolled into "cannon-balls"? If the bones had been buried in the indurated standstone they would have been penetrated by the indurating materials, which is not the case; and some of them would now be found with portions of the hard sandstone adhering to them, which also is not the case. Most of the bones are broken and have suffered some decay, and some have been eroded; but most of them show no erosion whatever. It would, too, be just as reasonable to insist that the human bones had been washed down out of the bog as that this had happened to the bones of the animals.

Attention has been called by Sellards to a number of cases in which several bones of one individual were associated. It is perfectly obvious that, if even a complete skeleton of an animal had been buried in a deposit, had there lost its ligaments and the animal matter, and had become partly fossilized, and then disturbed and transported, we may say even a quarter of a mile, the chances would be almost infinitely small that any two bones would find

lodgment near each other. Yet of the wolf (*Canis ayersi*), thirty or more bones were found at one place; and near by was the skull and femur of probably the same individual. Many bones of *Chlamytherium*, apparently belonging to one individual, were found at one place. Several bones of the jabiru (*Jabiru weillsi*), probably of one bird, were found close together. A jaw and large parts of the tusk of a mastodon were found in close proximity. Let us imagine a partially fossilized tusk of a mastodon or a delicate skull of a tapir being rolled a quarter of a mile down stream intact, while blocks of indurated sandstone are being ground down into globes. Thin turtle shells, softened by decay of the animal matter, could hardly endure transportation without destruction.

The cases just mentioned concern more especially materials found in no. 2. The writer has mentioned the fact that seven bones of an extinct snapping turtle (*Chelydra sculpta*) had been found together in no. 3. An illustration of these will be published in the forthcoming report of the Florida Geological Survey. Recently the writer has studied some deer bones found in no. 3. At one of the stations on the south bank of the canal and extending along it about 15 feet, were collected fifty deer bones; and while many of these are broken and some are injured by decay, none show signs of wear by water and sand. In this lot there are at least three individuals; but who can suppose that, if a skeleton of a deer had been buried in the bog already referred to, and had then been washed out, probably little by little, and swept down stream, any two bones would have been landed within fifteen feet of each other? The fifty bones of the collection would have represented as many individuals. Among these bones is a part of a left maxilla with three molars which fit accurately against the molars of a lower jaw. A second and a third dorsal vertebra seem certainly to have belonged to one deer. Three lumbars, the third, fourth and fifth, fit nicely together. A sacrum is complete and both innominata are present and join it accurately. Two radii belonged certainly to one young deer. One individual of the lot, probably all of them, belonged to an extinct species, *Odocoileus sellardsiæ*.

Chamberlin[1] in referring to similar cases, suggests that such

[1] *Symposium*, p. 39.

remains may have been buried along the banks in the upland deposit and, on being undermined, have fallen down and been reburied without suffering transportation. Unfortunately for this explanation, fossil skeletons do not appear to be any more abundant in that upland deposit, along the Van Valkenberg valley than they are in the old bog further up the tributary streams.

The writer believes that the theory of the secondary deposit of the fossil bones and teeth in the strata no. 2 and no. 3 is untenable and that we shall hear little more of it.

2. Let us now consider briefly the question whether or not the human bones had been introduced into the deposits no. 2 and no. 3 by burial at human hands. The writer believes that he is correct in saying that only Dr. Hrdlička has expressed an opinion to that effect. It is evident that Dr. MacCurdy does not share this opinion; for in his article in the *American Anthropologist*, on page 258, he indicates his belief that we can be sure that the oldest human skeleton and cultural remains at Vero are as old as the base of the upper deposit. It is the opinion of the writer that Dr. Sellards, in his last paper, has put the question of intentional burial of those human bones out of dispute.

It is not for us to assign limits to the wisdom of God, nor to the cunning devices of nature in the accomplishment of her ends, nor to the foolishness of men; but we do nevertheless seek for some gleams of reason in what human beings do. Can anyone now explain why savages who make pottery and flint implements and bury their dead would dig a grave in a water-soaked bed of muck, where masses of leaves and sticks and stems and even logs were likely to be encountered; when, by going a few rods away, they could make the grave in loose dry sand? And, if the burial be granted, how did it come about that the bones of the skeleton, even the elements of the skull, became scattered so widely? Why, too, did they in their migrations so generally seek the parting plane between the two strata? A sufficient explanation of what is found in the case of the human remains called no. 11 is that a skeleton or a part of one, probably in a more or less scattered condition, had been covered up in stratum no. 2, when this was being deposited,

not far above where the bones were recently found. After the bones had lain there long enough to become rotten, possibly somewhat fossilized, a freshet unearthed them and scattered some of them, including the thoroughly macerated and easily disarticulated skull, over the sloping surface of the bed of the stream. Apparently at once a lot of vegetable debris was thrown down on them, thus fixing their position. The writer believes that the theory of intentional burial of the bones in either no. 2 or no. 3 can not be defended.

3. Do any of the human remains which are found in no. 2 have the age of the deposit itself? The writer believes that the human bones are themselves as old as are the deposits in which they are found. This is the opinion of Dr. Sellards. Dr. Chamberlin says:[1]

> While this deposition of no. 2 was in progress the human bones are believed to have received their first and only burial in connection with the stream deposit.

Dr. MacCurdy, as already quoted, believes that all that we can be sure of is that the human remains date back to the interval separating the middle (no. 2) from the upper (no. 3) deposit.

It is believed by the writer that the bones found in no. 2 were covered in when the bed was being deposited because:

a. They have been found in that stratum in two different cases.

b. They are in practically the same state of fossilization as are the bones of the animals.

c. Not the slightest evidence has been produced to show that they were put there by human hands.

The circumstances connected with skeleton no. 11 have been discussed by Sellards, Hrdlička, MacCurdy and just now by the writer. MacCurdy in his last paper (p. 258) thought that Sellards in his paper of the same date had ignored this skeleton no. 1; but that is not wholly correct. While Sellards did not discuss this skeleton, he did refer (p. 248) to what he had already written about it. As to this skeleton nobody denies that the bones were found where reported. Nobody has attempted to show that there was any disturbance of the sand and marl above the bones; although the block of marl rock which capped the supposed grave was yet

[1] *Symposium*, p. 37.

there when the geologists and anthropologists visited the place. If, however, the anthropologist[1] believes that muck and sand, disturbed in digging and refilling a grave, will in time rearrange themselves so that the distinction between the two would be impossible, he would see little use in searching for a disturbance. Nobody has tried to explain in what way most of both legs and a little of both arms of that supposedly interred body might have been preserved and all the rest of the skeleton have disappeared. Had the skull, the vertebrae, the scapulae, and the pelvis been buried with the arms and legs, it is probable that some of them, or parts of them, would have been found either in the sand bed or lying loose at its base.

4. What is the geologic age of stratum no. 2? Before an answer is given to this question it will be necessary to discuss somewhat the geology and paleontology of the Pleistocene of North America.

It is now generally recognized by geologists that there were during the course of Pleistocene times four or five glacial and, corresponding to these, three or four interglacial, stages. These stages are presented in the following table, the names of the interglacial stages being italicized. A postglacial stage has been called by the writer the Wabash.[2]

10. *Wabash.*
9. Wisconsin.
8. *Peorian.*
7. Iowan.
6. *Sangamon.*
5. Illinoian.
4. *Yarmouth.*
3. Kansan.
2. *Aftonian.*
1. Nebraskan.

During the various glacial stages widespread deposits of till, with intermingled sands and gravel and boulders of northern origin, were laid down over portions of the Northern States. The accompanying map (fig. 1), the publication of which is permitted by the

[1] *Symposium*, p. 48.

[2] *Smithsonian Miscellaneous Collections*, vol., LIX no. 20.

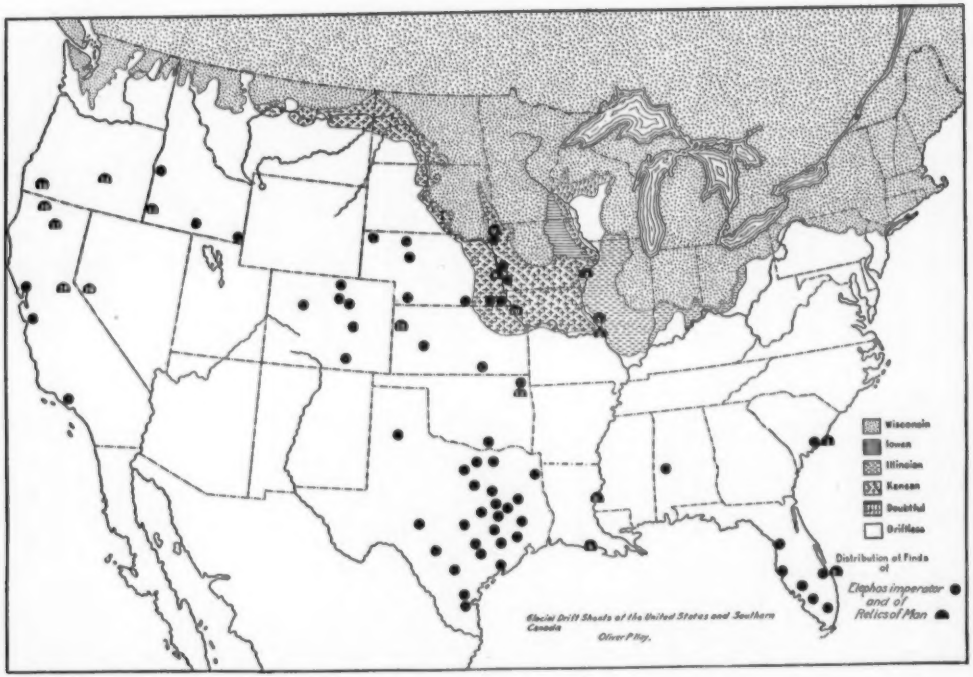
Wisconsin
Iowan
Illinoian
Kansan
Doubtful
Driftless
Distribution of Finds of
Elephas imperator and of Relics of Man
Glacial Drift Sheets of the United States and Southern Canada
Oliver P. Hay.

FIG. 1

Carnegie Institution of Washington, shows the general distribution of each of these glacial drift sheets, except the Nebraskan. This is hidden by later deposits, except where the latter are cut through by rivers. The interglacial stages are represented here and there in the region by deposits of various kinds, river gravels, sands, old soils, and beds of loess. All of these sometimes bear fossils and thus give us glimpses of the life of the times.

The most important of the fossil-bearing deposits found intercalated between deposits of glacial drift are the Aftonian gravels and sands of western Iowa. The Missouri river in this region has cut down through the Kansan drift so as to expose the Aftonian and Nebraskan beds. From this Aftonian, the earliest interglacial Pleistocene, there has been collected an important assemblage of fossil vertebrates. A list of the species has been published by the writer.[1] It includes three species of mastodons, three species of elephants, four or five species of extinct horses, a hipparion, an extinct bison, at least two undetermined species of camels, the Canadian beaver, the giant beaver, peccaries, mylodon, and megalonyx. In what have been known as the Equus, or more properly the Sheridan, beds in western Nebraska and Kansas an identical fauna has long been known. In the publication just cited and on the same page the writer has published a list taken from Dr. W. D. Matthew, of the species found at the type locality of the Sheridan beds, near Grayson, some miles from Hay Springs, Nebraska. To this list are to be added two horses and *Elephas imperator*.

There can be no doubt that the Aftonian interglacial deposits are simply a continuation eastward of the so-called Equus, or Sheridan, beds.

If, now, we study the collections made in the drift-covered regions east of the valley of the Missouri river, we must recognize that the mammalian fauna had at the time of its existence here undergone some changes. In northern Missouri and in southern and eastern Iowa there are large tracts which are covered by Kansan till or by this overlain by loess of different ages. In

[1] *Iowa Geological Survey*, vol. XXIII, p. 26.

Illinois, Indiana and Ohio, there are extensive regions which are covered up by Illinoian drift or by more recent interglacial deposits. Many of the animals which are found in the Aftonian and Sheridan deposits are found on or in some of the younger deposits just mentioned, such as *Elephas columbi*, *E. primigenius*, *Mammut americanum*, several species of *Bison*, *Tapirus*, *Cervalces*, *Mylodon*, *Megalonyx*, *Castoroides* and *Equus complicatus;* but no *Elephas imperator* has been found, no species of *Hipparion*, none of the many Sheridan and Aftonian horses except *E. complicatus;* no saber-toothed tigers and no camels. The region and the climate must have been favorable for their existence; and they would have left some traces of themselves if they had existed. From our present knowledge we can only conclude that they had perished.

When we come to examine the rather extensive collections which have been made in the deposits of the old filled-up lakes which lie upon the last drift sheet, the Wisconsin, we find that the vertebrate fauna had suffered further extensive changes. There are yet the two elephants and the mastodon, peccaries, the existing moose (*Alces*) and the extinct moose (*Cervalces*), also *Castoroides*, *Megalonyx*, and short-horned bisons; but there are no longer found any of the long-horned bisons of former times, no tapirs, no mylodons, and of course, none of the species which had dropped out of existence shortly after the first interglacial stage. It cannot be affirmed that the climate of our northern states did not permit the presence there of the missing species; for there was a period after the passing away of the Wisconsin ice sheet when peccaries lived along the shore of Lake Erie and in Michigan; and the megalonyx penetrated as far north as Minneapolis. What had taken place is what might have been expected; namely, that as Pleistocene times passed on one species after another dropped out of existence, while its place was either taken by other species or probably in most cases left vacant.

It may be well to take into consideration here the percentage of extinct species which are found at some of the places where considerable collections have been made. Of the species found in the Aftonian of Iowa about 90 per cent. are extinct; but as the deposits

are usually coarse, showing rapid currents, few of the smaller animals, such as rodents and insectivores, have been collected. Only the larger mammals are represented. The type locality of the Sheridan beds, at Grayson, Nebraska, has furnished about 71 per cent. of extinct forms. The cave at Port Kennedy, Pennsylvania, whose deposits are assuredly of early Pleistocene age, presents a fauna of which 80 per cent. of the species are no longer living. The conditions there were apparently favorable for the preservation of all kinds of land vertebrates; and the percentage may be taken as representing in a general way the composition of the early Pleistocene vertebrate fauna, as regards species yet living and those which have become extinct.

In a fissure in northwestern Arkansas, Barnum Brown secured an important collection of vertebrates. Of the mammals about 47 per cent. belong to extinct forms. On account of this low percentage, the absence of various species, and the presence of some forms of aboreal type, the writer concludes that the collection represents the Illinoian stage; but inasmuch as many low and small forms not so likely to be distinct from living species are present and many extinct species are missing which certainly were then living, it seems not impossible that the percentage is too low to represent the actual fauna. Naturally, after the Wisconsin stage had passed, relatively fewer extinct species appear and finally the fauna passes into that of the Recent epoch.

At this point reference may be made to the discussion which went on about the time (1890) when G. K. Gilbert published his monograph on Lake Bonneville. Cope and Marsh at that time held that the so-called Equus fauna and the deposits which contained it belonged to the Pliocene.[1] Gilbert, basing his views especially on the physical evidences, came to the conclusion that the fossil-bearing deposits around Lake Lahontan, Lake Bonneville, and Fossil Lake, Oregon (the latter furnishing what Gilbert accepted as a typical Equus fauna), belonged to the Pleistocene. This view has been accepted by vertebrate paleontologists in general. Gilbert, however, went further. Holding then that there had been

[1] Gilbert, *op. cit.*, pp. 397–400.

only two glacial stages and finding that the old lake deposits which furnished the Equus fauna belonged near the close of the later stage, he assigned the beds to the late Pleistocene. In this the writer believes that Gilbert was wrong. The fauna of Fossil Lake is identical with that found near Hay Springs, in the Sheridan beds. These beds, followed north, were found by W. B. Scott to pass beneath a drift sheet, which can be only the Kansan; while toward the east they pass likewise beneath the Kansan and reveal the same fauna in the valley of the Missouri river. Essential elements of it never reappear in deposits overlying the Kansan and later drift sheets. Between Gilbert and the paleontologists mentioned the honors were about equally divided.

Returning to the region of the Great Plains and making our way into Oklahoma and Texas, we meet everywhere with the Aftonian, or Sheridan, fauna. *Elephas imperator* (fig. 1), camels, several species of horses, some diminutive, others gigantic, tapirs and long-horned bisons, are found or likely to be found anywhere, even down to the border of the Gulf. Toward the east, camel remains have been found on Brazos river in Austin county, Texas; and a part of a skeleton of *Elephas imperator* was found by Dr. T. Wayland Vaughan close to the line between Texas and Louisiana. In some places this Pleistocene fauna is associated with remains of a species of mastodon belonging to *Gomphotherium*, a genus which was abundantly represented during the Pliocene. The genera *Mylodon* and *Megalonyx*, those old invaders from South America, have their ranks strengthened by the presence of *Megatherium*, *Nothrotherium*, and *Glyptodon*. It is surprising in studying the Pleistocene vertebrates of Texas how seldom remains of yet living species are met with.

In the low region along the lower portion of the Mississippi river from Louisiana to Alabama there have been found up to this time no *Elephas imperator*, no camels, no glyptodons, and only two species of horses. The fauna so far secured is approximately that of the post-Illinoian. The only exception to this is the association of *Equus leidyi* with *E. complicatus* in deposits at Natchez. Here too, is found a saber-toothed tiger, a species of which occurred

also in the Arkansan fissure already mentioned. Inasmuch as the muskox *Symbos cavifrons* has been reported from the same place it may be that the age of the deposits is Illinoian.

When we enter Florida the fauna which has been described as occupying the Aftonian beds (the so-called Equus beds of the Great Plains) and the coastal plain of Texas, reappears in full strength, but with slight modifications. There are present of the Xenarthra, *Megatherium*, *Mylodon*, *Megalonyx* and *Glyptodon*. *Chlamytherium* replaces *Nothrotherium;* and *Dasypus* is present. Of horses there are three distinct species, a large one, a medium-sized one, and a small one. Tapirs are represented by probably two or three species. Peccaries, hitherto unknown in the Gulf States, come to light at Vero. Camels are not abundant in the state, but are there. The genus *Bison* appears to have been represented by *B. latifrons*, the one with the longest horns, and probably by other species. Besides the common mastodon and the Columbian elephant, *Elephas imperator* has been found in several places. Great wolves and coyotes and foxes of extinct species are recognized. In addition to these there are many smaller creatures, some now extinct, many yet existing. There were many tortoises mostly belonging to extinct species and some of them were of immense size.

No one, it seems to the writer, can doubt that the fauna which has been described lived in the region west of the Mississippi in early Pleistocene times. There appears to be no good reason to doubt that it lived in Florida at the same time. Did it, now, continue to live there until quite recent times? Our friends the anthropologists appear to be willing to believe this. The writer holds that the view is wholly wrong.

a. It is improbable that a fauna composed as this was of, first, a native element, of another which had reached the country by way of the cooler regions of Asia, and of a third which had made an irruption from various parts of South America, a mixed assemblage of contending elements, subjected to the extreme climates of glacial and interglacial epochs and repeatedly to the pressure of tribes forced down from the north during glacial times, that such a fauna

would continue practically unchanged up into the Recent period and then suffer sudden extinction.

b. If species of *Megatherium, Mylodon, Chlamytherium, Glyptodon,* three species of horses, two or more species of tapirs, camels, species of long-horned bisons, *Elephas imperator,* and saber-toothed tigers lived in Florida after the close of the Wisconsin glacial stage, why did not some of these extend their range north as far as the northern half of Ohio, Indiana, and Illinois? Nearly all of them had at one time lived as far north as those states, and evidently the climate invited them; and the Wisconsin drift would not have burned their feet so soon after the ice had left it.

c. Why also did not these animals occupy even the lower part of the valley of the Mississippi? Along this river throughout Louisiana is found a fauna which was appropriate to about the middle of the Pleistocene; but the elements which distinguish the early Pleistocene are missing. If they were living so late as the Recent epoch in Florida their absence in Louisiana is astonishing. The answer to these problems is that the extinct animals so often mentioned did not live on up to the Recent epoch.

For the sudden wiping out of a great assemblage of such strange beasts as existed in Florida during the Pleistocene, the rapid deposit of sand and vegetable debris mingled with trunks of trees and the bones of animals and of men, and that at as late a period as 4000 years ago, the writer can think of no adequate agency except the Noachian deluge. According to the marginal readings of the authorized version of our Bible this event occurred 2349 years before Christ; that is, 4266 years ago. Archbishop Usher, a very learned man, was perhaps tainted a little by advanced ideas and was somewhat too liberal in his dispensing of time.

4. It appears to be possible at this point to answer the question asked on page 7: What is the geologic age of stratum no. 2? In view of the fact that about 70 per cent. of the mammals found in that stratum are extinct, and that these include species which belong with the fauna of the Sheridan and Aftonian beds and are not known to have existed at a later time, the stratum is regarded as belonging to the first third of the Pleistocene.

5. What is the geologic age of stratum no. 3 at Vero? In this stratum, known as the muck-bed, there is a lower percentage of extinct mammals, about 50 per cent. This appears to indicate a somewhat later time, about the middle of the Pleistocene. This lower percentage may, however, be due partly to what one may call accidental causes. The geologists regard the interval between the two deposits as having been brief.

Dr. Hrdlička[1] argued that a state of culture as advanced as that shown by the artifacts found in the deposits at Vero implies a numerous and widespread population and that such a population would surely have left many tangible traces of its presence on the continent, some of which at least would have been discovered by this time. Dr. MacCurdy[2] has expressed practically the same opinion.

The present writer is wholly willing to say that the conclusions reached by these distinguished anthropologists are very reasonable; furthermore, that the requirement which they make need not go entirely unsatisfied. The following cases are contributed; but they have long been accessible to everybody, and it is needless to say that our good friends the anthropologists have set up over most of them the danger signal. Let us set out from the Atlantic coast.

1. In the *Proceedings of the Academy of Natural Sciences* for 1859 (pages 178–179) Prof. F. S. Holmes gave an account of the exhumation of some bones, teeth and a nearly entire tusk of a mastodon near Charleston, South Carolina. These remains were found in sands underlying peat in draining a large swamp and at a depth of about 3 feet from the surface. Professor Holmes, with a small party of gentlemen, was present during the exhumation, and Holmes found immediately alongside of the tusk a fragment of pottery. This was said to be similar to that manufactured at the present time by the American Indians. Holmes stated that bones of a deer and two teeth of a horse were also found.

Naturally, somebody will say that this potsherd had reached its position by some accidental means. It might appear that a

[1] *Symposium*, p. 50.

[2] *American Anthropologist*, vol. 19, p. 261.

burrowing muskrat had been responsible; but, although muskrats were in that region in Pleistocene times, no muskrats have in Recent times been found anywhere near Charleston. The simplest and most probable explanation is that the pottery was dropped near that tusk about the time that the animal died. The presence of the horse teeth mark the time as being not later than about the middle of the Pleistocene. In similar deposits about Charleston have been found remains of tapirs, peccaries, mylodon, megatherium, elephants (among them *Elephas imperator*), hipparion, extinct capybaras, and probably camel. These indicate early Pleistocene age.

2. The discovery of human remains and artifacts at Vero naturally takes its place here.

3. In 1846[1] Dr. M. W. Dickeson exhibited before the Philadelphia Academy a large series of fossil bones which had been obtained by him at Natchez, Mississippi. Among these were the head of a megalonyx and parts of the skeleton of others, besides remains of a bison, an extinct bear, a horse, and apparently a mylodon. These had been found in a blue clay which underlies the loess of that region. Lyell, who afterwards visited the ravine in which the remains were found, stated that it was 60 feet deep. Dickeson, at the meeting referred to, exhibited also a part of a human innominate bone which he said had been taken out of the same blue clay, at a depth of 2 feet below the skeleton of the megalonyx. Naturally, efforts have been made to escape the conclusion that the human bone was as old as its apparent geological situation and its association with the extinct animals implied. Lyell thought that the innominatum had possibly fallen down the cliff from some Indian graves at its summit. Others have suggested that it had been washed down to that spot from some place farther up the ravine. The bone is yet preserved in the collection of the Philadelphia Academy; and it has been described and figured by Leidy.[2] He stated that it did not differ in any way in appearance and manner of fossilization from the megalonyx bones with which it was found.

[1] *Proceedings of Academy of Natural Sciences*, vol. III, p. 106.

[2] *Transactions of Wagner Institute*, vol. II, pp. 9–12, pl. II.

In 1892 Dr. Thomas Wilson[1] had his attention attracted to this human bone and proceeded to have a chemical analysis made of it and of a mylodon bone which had been left by Dickeson. Wilson stated that the color, texture, and general appearance of the mylodon bone had a remarkable similarity to that of the human bone. The analyses were furnished by Prof. F. W. Clark, of the U. S. Geological Survey, and they resulted in showing that the human bone had a content of over 22 per cent. of silica, while the mylodon bone had not quite 4 per cent. It would be interesting in this connection to have Indian bones from the summit of that cliff chemically analyzed.

A considerable number of species of extinct animals have been reported from the loess and deposits below it at Natchez, many of them apparently from the ravine mentioned above. In the opinion of the writer they indicate a middle Pleistocene period.

4. In 1866, Mr. J. F. Clew, of Petite Anse, Louisiana, called on Doctor Leidy and exhibited a piece of coarse matting which he affirmed had been found at Petite Anse, lying on the top of a newly discovered bed of rock salt, at a depth of about 16 feet from the surface, and immediately below some elephant bones. Clew further stated that there were in view other pieces of matting and other bones for anyone to see who would visit the place. Leidy tried to have the Philadelphia Academy send a competent man down there, but nothing was done in the matter.

Prof. Joseph Henry read a paper on the subject before the Chicago Academy of Science.[2] Nobody has ever questioned Clews's good faith in the affair. In 1890[3] J. F. Joor examined an extensive excavation which was being made to reach the rock salt. He found great quantities of pottery in a swamp muck, which had a thickness of 10 to 12 feet; but the pottery appears to have been in hollows which reached down from 3 feet to 6 feet from the surface. He concluded that Indians had been accustomed to boil down the brine which formed a spring there. He found a single piece of cane basket in a lump of mud, and he thought that it came

[1] *American Naturalist*, vol. XXVI, pp. 628–631.

[2] *Transactions of Chicago Academy of Sciences*, vol. I, p. 233.

[3] *American Naturalist*, vol. XXIX (1895), pp. 394–398.

from the lower part of the muck, or loam, below the level of most of the other human vestiges.

Below the loam was found a blue clay 2 feet or more thick. In this at a depth of from 16 to 20 feet from the surface, Joor found great quantities of bones of animals. These were afterwards studied by Professor Cope.[1] It appears from his communication and other reports that the following animals have been found at this place: *Megalonyx jeffersonii*, *Mylodon harlani*, *Equus complicatus*, *Odocoileus virginianus*, *Bison* sp., *Mammut americanum* and *Elephas columbi*. There are here no species which compel us to refer the deposit to the early Pleistocene. *Mylodon harlani* and *Equus complicatus* forbid the reference to late Pleistocene.

It has been suggested that Indians of recent times had sunken a shaft to the rock salt. Of this there is no evidence; it is improbable; and, if true, it is improbable that the tradition of it would have been lost. It has been thought, too, that such things as the matting, the pottery, and the animal bones had been washed down from the surrounding hills within rather recent times and covered up in the valley; but, while the bones are often broken and sometimes partly decayed, they do not show the effects of transporation. An explanation that appears to meet the requirements of the case is that about the middle of the Pleistocene, the valley was cut down so as to expose the rock salt; that men, probably Indians, carried away salt in cane baskets; that, later, through some slight change in level, material began to be deposited in the valley; and that, while this was going on, the mylodons and other animals lived and died and left their bones there.

5. It does not seem to the writer that the account given by McAdams[2] of the finding of a stone ax in Walkerville township, Greene county, Illinois, ought to be treated as suggesting a conspiracy of liars. Taken by itself the reported discovery would naturally have little weight; but if cases of the kind were common, no hesitation would be shown in accepting it. Dr. Shimek's objection[3] that there is no loess in that region which has a depth of 70

[1] *Proceedings of American Philosophical Society*, vol. XXXIV, pp. 458–461.

[2] *Proceedings of American Association for Advancement of Science*, vol. XXIX, p. 720.

[3] *Op. cit.*

feet is not decisive. Worthen[1] states that the loess on that county may be from 40 to 60 feet thick. Possibly McAdams was not an expert in identifying loess; and there are often differences of opinion among the experts. It is also possible that the well had penetrated the Illinoian drift and reached a still older deposit.

6. In the *Records of the Past*, vol. II, 1903, pp. 26–28, Cyrus A. Peterson, M.D., gave an account of the discovery of a stone ax in September, 1902, near Clayton, a village just west of St. Louis and nine miles west of the Mississippi river. This region is covered with a deposit of loess which at the place named has a thickness of from 10 to 15 feet.[2] During the construction of the belt line of railway around the city of St. Louis a member of the engineering corps picked up the ax in question after it had been turned up by the plow of the graders. The man in charge of the work removed the clay which enclosed the ax, and this was taken to his office and scrubbed clean. It then passed into the possession of the chief engineer. The ax was found at the juncture of the loess with the substratum of red clay, and at a depth of 14 feet from the surface. It appears not to differ from Indian axes of recent times. There appears to be no reason whatever for questioning the authenticity of the discovery.

7. In 1891 Prof. F. M. Witter, of the public schools of Muscatine, Iowa, read a paper[3] in which he told of the finding of arrow points in the loess at Muscatine, Iowa. In one case a brickmaker reported that he had taken a chert point from the loess at a depth of 12 feet from the surface. At another time the same man took an arrow point from the same bank at a depth of 25 feet from the surface. Dr. B. Shimek[4] in speaking of this case refers to it as "Witter's cautious references to the discovery, by another party, of arrow-heads under extremely doubtful circumstances." As a matter of fact Witter investigated the matter as well as probably any one could and he expressed no doubt as to the authenticity of the discoveries. There was a third arrow point taken by the same

[1] *Geological Survey, Illinois*, vol. III, p. 123.

[2] Fenneman, *U. S. Geological Survey, Bulletin 438*, p. 33.

[3] *Proceedings of Iowa Academy of Sciences*, vol. I (1892), pt. 2, p. 66.

[4] *Bulletin of Geological Society of America*, vol. XIX, p. 244.

brickmaker from another brickyard in which there was a blue clay whose upper surface was 8 feet from the top of the ground. This arrow point had passed through the mixing bed before it was discovered. Witter examined the bank and inquired into the circumstances and wrote "I believe with Mr. Freeman, that the arrow point must have come from the blue clay." There is no intimation whatever that he doubted the word of the brickmaker in respect to any of the discoveries. The loess has been regarded as a deposit of the Sangamon stage; but, inasmuch as an extinct reindeer, *Rangifer muscatinensis*, has been found it it, a cold climate is indicated. One might not be far out of the way to assign the time to the Iowan glacial stage.[1]

In the publication of 1892 above referred to, Witter reported other artifacts found near Muscatine. About a mile above its mouth, Mad Creek had cut away a point of a hill, leaving a bluff almost perpendicular and about 40 feet high. The top was covered by loess. About 10 feet from the summit was a bed of gravel one foot thick, from which a student had taken a considerable part of a tooth of an elephant. Witter examined this bed and in doing so found in it numerous flint chips such, he adds, as are supposed to have been struck from arrow and spear points, knives, etc.

8. In his report on the geology of Pottawatamie county, Iowa, Dr. J. A. Udden[2] reported the alleged discovery of a stone ax in the loess at Council Bluffs. It was met with in tunneling for the cellars of a brewery, under 30 feet of loess and at a distance of 40 feet from the mouth of the tunnel. Udden stated that the ax had on one side an adhering incrustation of calcareous matter that had evidently been deposited by ground water. The ax had been discovered by workmen and immediately turned over to the engineer who had charge of the operations. It is hard to see here any motive for deception on the part of anybody. Udden found no evidences of disturbance of the loess at the place.

9. In 1876 Dr. Samuel Aughey[3] wrote that he had found, three miles east of Sioux City, Iowa, a small arrowhead in the loess;

[1] Udden, *Iowa Geological Survey*, vol. IX, p. 360.

[2] *Iowa Geological Survey*, vol. XI, p. 261.

[3] *Annual Report, U. S. Geological Survey of the Territories* (1874), p. 254.

but he did not furnish any details. He stated further that he had found, two and a half miles southeast of Omaha, in a railroad cut, at a depth of 20 feet in the loess, and at a distance of at least 6 inches from the edge of the cut, a large coarse arrow or spearhead. It is fair to say that Doctor Shimek[1] characterizes these statements as absolutely unreliable; but the reasons are not given.

10. In 1907[2] Miss Luella A. Owen reported the discovery of a chipped implement in the loess near St. Joseph, Missouri. The implement is described as being composed of a fine, close-grained trap-rock. It was taken out of a perpendicular wall of the loess, along a highway, by an attorney of St. Joseph who was interested in collecting such objects. It was firmly imbedded in the loess, with the point directed nearly downward, at a height of 10 feet above the road and not less than 20 feet below the natural surface. The one side turned somewhat upward was partly coated with oxide of iron. It might, of course, be asserted that at some time in the past there had been a crevice here into which the ax had fallen. The numerous applications of this explanation tend to show that Indian implements have contracted a habit of waiting for such opportunities.

11. An interesting occurrence of flint implements in association with remains of extinct animals has been described by Prof. W. H. Holmes, of the United States National Museum.[3] From a sulphur spring not far from Afton, Oklahoma, Professor Holmes secured more than 800 specimens, including fragments of arrowheads, spear points, knives, etc.; and he estimated that as many more had been previously removed. This collection of beautiful flints is now in the National Museum. Nearly the whole lot was found piled together at the bottom of the spring at a depth of from 4 to 7 feet. In the same spring were found, according to Holmes' report, many teeth and bones of mammoth, mastodon, horses, buffalo, deer, elk, and wolf. The greater part of these were found within a radius of 3 feet from the spring basin, at a depth of from 4 to 7

[1] *Op. cit.*, p. 244.

[2] *Records of the Past*, vol. VI, pp. 289–292.

[3] *American Anthropologist* (N. S.), vol. 4 (1902), p. 108. *Report of U. S. National Museum* (1901), p. 237.

feet. Within this area were found at least 100 mastodon teeth, 20 mammoth teeth, and many teeth of horses and bisons. Professor Holmes came to the conclusion that Indians who had inhabited the region had regarded the spring as sacred and had cast the flint implements and possibly many of the bones and teeth into it as offerings to the divinities of the spring. It is to be noted that the presence of all these objects indicates that the vent of the spring had not changed position for a very long time.

The present writer has had the opportunity to study the collection of bones and teeth which was made at Afton, and he finds that it includes 19 distinct species of mammals. Among these are *Castoroides ohioensis*, four species of extinct horses, two species of camels, the extinct moose *Cervalces*, the extinct muskox *Symbos cavifrons*, *Elephas columbi* and *E. imperator*. Of the latter there are several beautifully preserved teeth. Including in the estimate the common bison and two wolves, which may have got into the spring within comparatively recent times, we find that two thirds of the species are extinct. In short, we have here again the Sheridan, or Aftonian, fauna which is so often met with all over the Plains region. That these remains were cast into the spring by human hands is out of the question; for such objects are undoubtedly very rarely found on the surface in that region; and they are, according to Holmes' investigations, met with in the deposits around and at a distance from the spring. Nor does the writer believe that those bones and teeth that were found in the spring belonged necessarily to animals that died within a very few feet of the spring.

It seems perfectly evident that a stream of water rising from the earth must continually wear away the walls of the orifice, which does not need on that account to become larger. The earth near by will be pressed toward the opening and will keep its size reduced. The effects of long-continued pressure of the earth on retaining walls may easily be observed. The result will be that there will be a slow but unceasing movement of the ground and whatever is in it toward the spring. Once within the spring, the fine materials will in time be swept away, while the bones and teeth, if any, will

remain there until they are worn out or decay. If that sand deposit at Afton was laid down during the first third of the Pleistocene, it is not strange that so many bones and teeth were found in the spring itself. Professor Holmes himself, in a way, recognized the possibility of this movement of the ground.[1]

If we assume that the diameter of the vent was two inches and that a layer one half inch in thickness around the vent was carried away in a year, an estimate will show that in 100,000 years all the earth within a radius of about 30 feet would have been brought up to the spring and swept away.

As already said, Professor Holmes has regarded the flint implements found at Afton as the offerings of Indians to the god of the spring; and he has furnished some evidence in support of his view. It is impossible to refute this opinion; but the writer does not see why the testimony of superannuated Indians in this case should be regarded as having greater value than that of honest white men, in other cases. And there may be found some other explanation of this matter. Possibly early Pleistocene Indians, lying in wait to get meat for their families, shot arrows which missed the mark and fell into the muck. In time, like the teeth and bones, they were carried into the spring and during long ages accumulated there. They were sharpened to keen edges, perhaps not to touch the hearts of the divinities of the spring, but to pierce the tough hides of the buffaloes. Against the theory that the flints were thrown into the spring is the fact that one implement was found in the deposit of sand at a depth of 3 feet from the surface of the ground and 3 or 4 feet from the curbing which was put down in the spring. At any rate, we have here another intimate association of artifacts of human beings with remains of early Pleistocene mammals, and this may mean their contemporaneous existence.

12. In 1902[2] Dr. S. W. Williston reported that in 1895 Mr. T. Overton and Mr. H. T. Martin, assistants in the paleontological department of the University of Kansas, had discovered an arrowhead lying beneath the scapula of a fossil bison, in Logan county,

[1] *Op. cit.*, p. 242.

[2] *American Geologist*, vol. XXX, pp. 313–315.

Kansas. At this place the skeletons of five or six adult bisons were found within an area of about 10 feet square, besides the skeletons of two or three young ones. Overlying these skeletons was 20 feet of the so-called plains marl. Mr. Martin has informed the writer that the bank where the first of the bones was seen was perpendicular and that it was 12 feet farther in where the arrowhead was found. Dr. Williston had long known Mr. Martin, who first saw the weapon; and he had not the slightest doubt of the reliability of the testimony given by him and Mr. Overton.

Now, the fossil bisons found at that place belong to *Bison occidentalis*, a species which has left its remains at a good many localities in our country. None of these remains, however, has ever been reported from any deposit which overlies Wisconsin drift. The skeletons found in Logan county may therefore be as old as the Aftonian or may belong to any succeeding pre-Wisconsin stage.

13. A brief reference must be made to what is known as the Nampa image. In 1889 an artesian well was being bored at Nampa, Idaho, a town on the Oregon Short Line railroad. The well was closely tubed by a six-inch pipe. While materials were being brought up from a depth of about 320 feet, there appeared an image an inch and a half in length which represented a woman and which was made of baked clay. A brief account of this discovery is given in G. F. Wright's *Ice Age in North America*, page 701, with a figure of the object. In the *Proceedings of the Boston Society of Natural History*, volume XXIV, 1890, pages 424–450, appear the discussions of the investigations which were made on this image. The possibility of any accidental or fraudulent intrusion of the object appears to be excluded.

In arriving at the depth noted above, the drill passed through 60 feet of soil, then from 12 to 15 feet of lava rock; while below this were deposits of various kinds. Naturally our anthropologists are inclined to question the authenticity of this discovery, especially on account of the great age that it indicates for a rather advanced race of men. Wright accepts the object as genuine, but regards the deposits as rather late Pleistocene.

In the Nampa folio, no. 103 of the United States Geological Survey, a section of the well is given. Reference is made to the discovery of the image. The authors say that "further confirmation of this occurrence has not been forthcoming and the image may have been dropped into the wellhole by some one wishing to perpetrate a practical joke." This statement is to be compared with the discussion before the Boston Society as cited above.

The beds passed through in this well are referred by the authors of the folio to the Pliocene. From deposits within the quadrangle believed by them to be of the same age they reported remains of *Equus*, *Procamelus* (probably *Camelops*), *Mammut*, a mastodon which was different from *M. americanum* and which therefore probably belonged to *Gomphotherium*, besides some other apparently extinct mammals, and an extinct swan. Here we have once more the Equus, or Sheridan, or Aftonian fauna, and the deposits certainly belong to the Pleistocene.

14. Brief mention will be made of the supposed discovery of arrow and spearheads of obsidian at Christmas lake, Oregon, in 1877, as reported by Professor Cope.[1] At this place there was found an abundant fauna consisting of mammals and birds, and belonging, as Cope then held, to the Pliocene. Mingled with these remains were found numerous obsidian implements. Naturally, efforts have been made to show that these weapons were of recent origin; and Cope himself[2] admitted that the evidence was not absolutely conclusive. Nevertheless, the other view may well be the correct one. It appears not improbable that the bones and flints had rather recently been brought to the surface by the action of the winds.[3]

15. Attention may be called to an article written by Prof. J. F. Kemp, of Columbia University, in 1906,[4] in which he details the finding of mortars and pestles in auriferous gravels at Waldo, Josephine county, Oregon. He stated that these occurrences add

[1] *American Naturalist*, vol. XII, p. 125.

[2] *Proceedings of Academy of Natural Sciences, Philadelphia*, 1882; *American Naturalist*, vol. XXIII, p. 979.

[3] Gilbert, *Monograph* XI, *U. S. Geological Survey*, p. 394.

[4] *Science*, vol. XXIII, p. 435.

their contribution to the general mass of evidence regarding the antiquity of men in the far west. He also expressed his realization of the criticism which is usually brought to bear on such reports.

16. In 1880[1] Professor Cope reported that during the summer of 1879 he had obtained bones of *Mylodon* from the auriferous gravels of Klamath river, near Yreka, California, from excavations which he had personally examined. He secured also vessels of versicular basalt, which, he said, had been undoubtedly procured from the same excavations. Of course, there was some chance here for the perpetration of a joke on a paleontologist, but Cope was a man not easily imposed upon.

17. We ought not to pass by the discoveries made in the caves of northern California by Dr. John C. Merriam and his students, Dr. W. J. Sinclair and Mr. E. L. Furlong. These discoveries were discussed by Doctor Merriam and Prof. F. W. Putnam in the *American Anthropologist*, volume VIII, 1901, pages 221–228; 229–235. In Potter creek cave Dr. Sinclair found 45 species of mammals, of which at least 21 were considered extinct; that is about 46 per cent. The list was presented by Sinclair in 1904.[2] There are in it two species of horses, apparently two of *Megalonyx*, a bison, a camel, the remarkable artiodactyl *Euceratherium*, the extinct wolf *Canis dirus*, and the extinct bear *Arctotherium simum*. The fauna is regarded by Sinclair as of the same age as that of Fossil Lake, Oregon, and that found near Hay Springs, Nebraska.

Now, associated closely with these remains were many splintered bones, some of which seemed to have been shaped and polished by human hands. Regarding these Professor Putnam remarked that it seemed impossible to explain their presence excepting by the agency of man. Dr. W. D. Matthew of the American Museum of Natural History and J. W. Gidley examined these bones and reported that in their opinion the only explanation of the perforations was that they were the work of man.

In Samwel cave was found a fauna which was regarded as somewhat younger than that of the Potter creek cave. In it were found

[1] *American Naturalist*, vol. XIV, p. 62.

[2] *University of California, Publications in American Archaeology and Ethnology*, vol. II, p. 17.

split and polished bones similar to those mentioned; besides these were secured a chipped fragment of basaltic lava and a chipped fragment of obsidian. These were, however, not seen in place.

18. Table Mountain, in Tuolumne and Calaveras counties, California, has for more than a half century been a storm-center of debate. For information and opinions the reader may consult J. D. Whitney,[1] G. F. Becker,[2] W. H. Holmes,[3] W. J. Sinclair[4] and G. F. Wright.[5] The contention regards the alleged discovery of remains of man and his works in the auriferous gravels, and the age of the remains. Whitney, Becker, and Wright affirm the sufficiency of the evidence; Holmes and Sinclair deny this. The two latter writers certainly have shown great skill in throwing doubt on all the finds. Fortunately, all the authors agree that Table Mountain is a reality.

Professor Holmes appears to base his objections to admitting the authenticity of the discoveries on the idea that they would prove the existence of a race of Tertiary Indians as advanced as those of today. This view is certainly incorrect. The mammals that have been mentioned in connection with the discoveries in that region are rhinoceros, elotherium, hippopotamus, mammoth, mastodon, tapir, camel, horse, bison, and deer. Certainly the first two do not belong with the others, as Cope long ago pointed out; and they are not known to have had anything to do with any human remains. As to the hippopotamus, there was some error. The existence of any animal of the kind in this country has not been confirmed. The other animals are members of the Sheridan fauna so often mentioned in this paper; and, so far as we know, it existed during the first interglacial period of the Pleistocene. If these human remains and artifacts and those animals have been found in those auriferous gravels, as the writer believes the evidence taken as a whole indicates, we do not have to accept a Tertiary man, but one of early Pleistocene times.

[1] *Memoirs of Museum of Comparative Zoology, Harvard University*, vol. VI (1879).

[2] *Bulletin of Geological Society of America*, vol. II (1891), p. 189.

[3] *Annual Report, Smithsonian Institution for 1899*, p. 419.

[4] *University of California Publications in American Archaeology and Ethnology*, vol. VII (1908), p. 107.

[5] *Ice Age in North America* (1911), p. 687.

It must be added that as accomplished a geologist as Clarence King extracted by his own hand a pestle out of the auriferous gravels beneath the basalt capping at Table Mountain, as is related by Becker. Sinclair's paper may be consulted for remarks on this discovery.

19. In 1889[1] W J McGee published a paper on his discovery of an obsidian implement in the wall of a canyon of Walker river, Nevada, at a point about fifteen miles from Lake Walker. The implement was found sticking point outwards in the old lake silt at a distance of 25 feet from the summit. McGee examined carefully the condition surrounding the object and was unable to find any reason for believing that it had been introduced after the laying down of the silt. His discussion of the subject is commendable. In the same deposits of ancient Lake Lahontan he found bones of elephant, bison, horse, and camel. In I. C. Russell's work on Lake Lahontan[2] reference is made to McGee's discovery. The bones and implement were buried in the clays, which were regarded by McGee, G. K. Gilbert, and Russell as having been laid down during the waning of the later of the two glacial epochs which they recognized and as thus belonging to a time not far removed from the Recent.

On the other hand the presence of horse and camel remains must push the age of the clays back to the first third of the Pleistocene, apparently not later than the close of the Kansan glacial stage. In what are deposits of probably the same age laid down in old Lake Bonneville have been found remains assigned to *Bison latifrons*.[3] The remains probably belonged to another species of *Bison*, but they indicate an age at least pre-Wisconsin.

It seems apropos to cite here another testimonial to the fact that man lived in the New World during early Pleistocene times. Some years ago, in a dry cave in southernmost Patagonia, there were found a part of a skin and many bones of the great ground-sloth *Grypotherium*, related to *Mylodon*. There were found evidences that men had occupied the same cave and had possibly

[1] *American Anthropologist* (N. S.), vol. 2, pp. 301–312.

[2] *Monograph* XI, *U. S. Geological Survey*, pp. 247–269.

[3] King, *U. S. Geological Exploration of the 40th Parallel*, vol. I, p. 494.

domesticated the sloth. Associated with these remains of the sloth and of man were those of various extinct Pleistocene animals. Dr. A. S. Woodward concluded a paper[1] by saying that zoologists and geologists could not fail to agree that the ground sloth belonged to the Pampean fauna, and that they could hardly refuse to believe that the animal was actually kept and fed by an early race of man. The Pampean fauna belongs to the Pleistocene.

It seems to the writer that the cases above cited testify strongly to the fact that during early and middle Pleistocene times there existed in North America a population which was spread from the Atlantic Ocean to the Pacific; and that it left many tangible traces of its presence.

When Thomas Wilson, as above cited, called attention to the significance of the high degree of mineralization of the human bone found at Natchez, he made the judicious remark that one swallow does not make a summer. It may not be wrong to suggest, however, that when they come in flocks one has a right to conclude that at least the vernal season is on.

Doubtless in almost every case of discovery of an object buried in the earth some weak point can be found in the evidence; for the conditions are always complex. Most discoveries are made accidentally and by men who do not know the significance of the objects, nor what observations to make and record. One might wish that such men could have a presentiment of the discovery that was looming and could have at hand a geologist or a physical anthropologist to witness its advent; but it appears that expert geologists receive little more consideration than expert well-diggers. It seems to the writer that our friends the anthropologists give too little weight to the cumulative effects of the reported discoveries of Pleistocene man. That juror would render a bad verdict who would direct his attention to the weak points of the testimony of a lot of witnesses, to their stupidities, evasions and contradictions, and not regard the consistent story which ran through the testimony of all.

Among the most familiar objections offered against the authen-

[1] *Proceedings of Zoological Society of London* (1900), p. 78.

ticity of reported discoveries of early man are these: There was no scientific expert present; there were no witnesses to corroborate the statements of the finder; the object may have fallen down from some later deposit to the spot where picked up; it and the accompanying fossil bones may have been washed down quite recently from some older deposit; it may have worked its way down through a crevice or through some animal's burrow or by a hole left by a decayed root; some seeker after notoriety or an unscrupulous collector may have tried to impose on the public; or the ubiquitous joker may have exercised his wits on an unsophisticated geologist or anthropologist. Certainly all these are possible geological agencies and must be taken into account; but to what extent do they operate?

What is to be said about the geographical and stratigraphical distribution of alleged discoveries of early man in this country? A great sweep of territory extending from the Atlantic to the Pacific, including large portions of our most densely inhabited states is covered over with drift deposits belonging to four or five glacial stages. Into this drift materials there have been sunken innumerable cellars and dug wells, many shafts to reach coal and to prepare foundations for bridges and large buildings. In numbers unreckoned there have been wells bored to reach water and gas and petroleum. Railways have been cut through thousands of hills composed of glacial till. Along nearly every river and brook within the glaciated region are found banks, often many feet high, which offer fine sections of one or the other of the different glacial sheets. In this region gravity acts as elsewhere and so does the transporting power of water. The drift may be about as liable to crack and slump as is the loess. In this region the trees may or may not send down as deep their roots as in the loess of Missouri or the muck beds of Florida, but they do penetrate the drift. The gophers, prairie dogs, woodchucks, and badgers living on the drift are doubtless just as industrious as elsewhere. It is not to be doubted that every man who knows anything about geology, or paleontology, or has any inkling of anthropological questions has scanned eagerly the sections of the drift along our railroads and

our streams in the hope of finding there some relic of former man. Now, how many discoveries of such relics have been reported as having been made in the undisturbed drift of the ground moraines and terminal moraines? With perhaps a single exception, the writer has been able to learn of none. The literature of the subject has been pretty well examined; and application to eminent glaciologists has been made and no cases have been discovered. The exception made is the discovery reported by G. F. Wright;[1] but here too the find was in gravel along a river, and this gravel was possibly reworked glacial materials. Even in a terminal moraine, where the glacial ice alternately advanced and receded, there might be found relics of some hardy race which lived close to the ice front; but men could not have lived under the glacier. It is for our friends the anthropologists to reflect why discoveries of human remains and artifacts are not reported from unmodified drift deposits. Why do the geological agencies which have been invoked so often in other regions not act also in the glacial till? It looks as if their value was reduced to almost negligible quantities. And one might further inquire whether, when they live and move on glacial drift, rascally collectors become more honest or more circumspect; jokers less facetious; and geologists and anthropologists less credulous?

Over large portions of our country south of the glaciated region there is spread out a mantle of materials which have resulted from the weathering of deposits of various ages. These constitute the greater part of Dr. W J McGee's Lafayette formation. They consist of gravels, sands, and clays and cover an area of more than 200,000 square miles; and they extend from near the coasts of the Atlantic Ocean and the Gulf of Mexico to elevations of from 500 to 1500 feet. The areas occupied by these deposits were formerly well populated by Indians; and the materials appear to be well adapted to receive artifacts through the accidental agencies which have been mentioned above. Moreover, in many places the materials have been considerably eroded, so that such inclusions might be brought to view. The writer has not been able to learn

[1] *Ice Age in North America*, pp. 651–653.

that any one has ever set up a claim to have discovered in this so-called Lafayette any traces of early man.

It appears to the writer that the matter may be summed up thus: In Pleistocene deposits laid down under conditions which permitted the existence of man we find numerous evidences of his presence. In Pleistocene deposits originating under conditions forbidding the presence of man and in older accumulations no traces of man's existence are met with, even though both those of Pleistocene age and the older ones are adapted to receive adventitious inclusions of artifacts.

The opposition that is presented against the idea that a race like our present Indians lived in our country during Pleistocene times is based on the views that European anthropology furnishes us a scale by means of which may be measured the time and the manner of development of the human race over the whole world. Now, it is not at all probable either that the human race had its origin in Europe or that the rest of the world received its populations from Europe. It is far more probable that man appeared first in Asia; perhaps, as Arldt[1] suggests, on the Thibetan plateau. From this center, the races streamed forth in all directions, somewhat as is shown on Arldt's plate 23. It is improbable that the Heidelberg and Neanderthal men had anything to do with the present human races, except in having had, somewhere back in the Tertiary, an origin in common. They represented species of beings different from *Homo sapiens*, species that attained a certain stage of bodily and mental development and then were exterminated by the superior races. If they too were of Asiatic origin, they were swept westward by the advancing waves of more intelligent tribes until reaching Europe they for a time held their pursuers at bay. Meanwhile the superior races were probably populating the other continents and the islands of the sea; and this occupation probably went on during late Pliocene and early Pleistocene times. We cannot avoid the conclusion that about the close of the Pliocene and probably at different times during the Pleistocene there was free communication between the lands of northeastern Asia and northwestern

[1] *Die Entwicklung der Kontinente*, p. 606.

America. Over the wide land-bridge existing there the animals of Asia swarmed into America and American animals into Asia. If man existed in Asia at that time, there appears to be no reason why he should not have accompanied on this journey the beasts with which he had associated.

It is to be noted at this point that there is by no means agreement in Europe among the anthropologists on the one hand and geologists on the other, regarding the time when the first men-like creatures reached Europe and regarding the glacial and the interglacial stages during which the various primitive races flourished in that country. The early cultures which by the anthropologists are put in the third interglacial are relegated by Penck, Geikie, and Wiegers to the second interglacial stage; and by Wiegers the pre-Chellean is put back into the first interglacial.

We have now to inquire whether or not a race of men with about the grade of development of our North American Indians would have been out of harmony with the rest of the mammalian fauna during the first interglacial stage in North America, that known as the Aftonian. The writer believes that the ancestor of the present races who should have existed during the first third of the Pleistocene in a stage represented by the Heidelberg man would have been an anomaly. It seems evident that not another land mammal belonging to the Quaternary epoch of North America or Europe has undergone anything like the profound changes which man would need to have undergone in order to make such ancestry possible.

At the outset, the collections which have been made in early Pleistocene deposits at various places in North America show from 20 to 50 per cent. or more of species which still exist, and which have undergone no changes that are perceptible. Among these may be mentioned the Virginia deer, the beaver, the brown bear, the raccoon, the glutton, the gray wolf, the coyote and the gray fox. Possibly with more complete materials some of these might be separated specifically. Other species are with difficulty distinguishable, or not at all, from existing ones, as one of the fossil tapirs. Many others are certainly specifically different, but the differences

are not great. There are several species of early Pleistocene horses; but when one has the skull and even the skeleton of one of these the characters distinguishing it from the existing horses are often not easy to discover; and one must often fear that he may have had palmed off on him the remains of the domestic horse. As regards the development of the brain, that of a horse from the Sheridan beds is as large as that of the domestic horse or even larger. The early Pleistocene bisons resembled greatly our existing bison, but had longer horns. The extinct tapirs of the early Pleistocene differed little from those now living; and the same is true of various feline and canine forms. The giant beaver, *Castoroides ohioensis*, appears to have lived from the beginning to the end of the Pleistocene. Just why it did not continue on to our day is not known. The same remark is true of the American mastodon, of *Elephas columbi*, and *E. primigenius*. Similar observations may be made regarding the stage of development of the early Pleistocene animals of Europe. Why should it be supposed that our ancestors were so retarded in their evolution until past the middle of the Pleistocene and that this evolution then should have suddenly been quickened? The various divisions of the Pleistocene differ from one another and from the Recent epoch principally in the successive extinction of animals, especially of the large and striking species; and not so much in changes in structure. Taking into consideration the rather unimportant morphological advancement made by the great majority of vertebrates since the early Pleistocene and the extinction of so many highly differentiated forms one might be pardoned for entertaining the view that on the whole the vertebrates have lost ground.

It may be permitted now to inquire whether the nearest relatives of man, the higher apes, have undergone important structural changes since the geological epoch when they first present themselves to us. In the Siwalik deposits of northern India, which belong to the latter part of the Miocene or early Pliocene, there has been found a palate of an ape which Lydekker referred to the genus *Troglodytes* (now *Pan*), the type of which is the existing chimpanzee. Lydekker stated that the teeth differed from those of the chim-

panzee in presenting a still more marked approach to the human type of dentition. In the same deposits was found a canine tooth of an ape which resembled so closely the corresponding tooth of the orang-utan that it was impossible to distinguish the two. There existed a species of *Semnopithicus* whose molars were extremely like those of the existing proboscis monkey. Two species of *Cynocephalus* were discovered, with regard to which Lydekker remarked that it could not be certainly affirmed that they were distinct from yet existing species of baboon. If these animals had at that early time reached such a high grade of development and if this has been maintained without important change to our day, why should it be thought a thing incredible that a race of men had attained in the first third of the Pleistocene the native American level and that it has not departed greatly from it up to the present?

The anthropologists who have discussed the discoveries made at Vero have impressed us with the fact that the human bones and the implements are identical with those of rather recent Florida Indians. They are the men to decide this question and the rest of us must submit. However, Dr. MacCurdy in his last article on the subject, indicates that this is not the whole of the matter. The bone points, he tells us, may be duplicated from southern and far southwestern mounds. The bone fish-hooks are exactly such as have been found in Connecticut. The same, he says, is true regarding the pottery. Might he not also have extended the range of similar objects both in space and time, in space to considerable parts of the Old World and in time well back into the Pleistocene of Europe? Is it not true likewise that, while the human bones resemble those of recent Florida Indians, they resemble also those of most other Indians? Under the circumstances, it does not appear to be necessary to conclude that these bones and artifacts were derived from Florida Indians living within 4000 years.

If the reader will consult a paper written by H. Klaatsch[1] he will find that in Tasmania and Australia the majority of the artifacts were similar to those of Mousterian times in Europe. In Australia were found chipped implements which were quite like

[1] *Zeitschrift für Ethnologie*, vol. 40 (1908), pp. 407–428.

those referred in Europe to the Chellean and Acheulean; others were even more primitive than the supposed eoliths of the Miocene and Oligocene. It is evident, therefore, that some races of men stand still in their culture or move so slowly that their progress is imperceptible.

Finally, Dr. MacCurdy offers as evidence against the great age of the pottery found at Vero the fact that not even the rudest pottery is found in the argillite culture in the Delaware valley. This argument loses its force when it is considered that even within historical times many Indian tribes have had no knowledge of pottery.

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THE GROWTH OF BOYS: DENTITION AND STATURE

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STUDIES of growth usually give measurements of individuals based on their actual or "chronological" ages and not on their physiological status. The following relations of stature to status as defined by dentition are based on plaster casts of the dental arches and measurements of some three hundred and fifty school-boys of Utuado, Porto Rico; collected by Professor Franz Boas in 1915. The method of treatment was suggested by Professor Boas.

It has been shown[1] that the observed characteristics of curves of growth may be explained as due to variations in the period of development, *i. e.*, to accelerations and retardations. Since the stage of development of a child, at any period, depends on the previous acceleration or retardation of its growth, which affects the body as a whole, it follows that all measurements of the child must vary together; either all values must be increased beyond the average of its age by acceleration or all depressed by retardation. The more rapid the rate of growth, the greater is the effect of variation in period on all the measurements, that is, correlation between all measurements increases during the period of rapid growth and declines during the period of decreasing growth. The variation in period as expressed by a body measurement must be the variation of period at which a particular physiological status is attained. We find, therefore, a correlation between observed physiological status and body measurements. Such a correlation of pubescence to stature, weight, strength of the right hand and to mental development as indicated by school standing has been shown by Crampton,[2] to head length and width and to width of

[1] Boas, Franz, and Wissler, Clark, "Statistics of Growth," *Report of the Commissioner of Education for 1904* (Washington, 1905), pp. 25–132.

[2] Crampton, C. Ward, "Physiological Age—A Fundamental Principle," *American Physical Education Review*, vol. XIII (Springfield, Mass., 1908), pp. 141, 214, 268, 345.

face by Boas,[1] and Deaver's data indicates the same relation of pubescence to strength of the forearms and shoulder retractors and to lung capacity.[2] We should expect a similar correlation of physiological development as indicated by dentition with stature.

Crampton has demonstrated a correlation of dental development with stature and weight. Among 934 New York boys, at a certain age individuals were taller and heavier in proportion to the number of permanent canines and second molars they had erupted.[3] In this study the eruption of corresponding upper and lower teeth is considered as a single phenomenon. However, there is usually an interval of about a year between their eruption; therefore, these results are somewhat invalidated. While the present series is smaller, about 350 individuals, the study has been extended to all the deciduous and permanent teeth.

Physiological periods for the Porto Rican boys may be obtained from the data on dentition. The following tables show the number

TABLE I

NUMBER OF DECIDUOUS TEETH PRESENT

Age	Cases	Upper Jaw					Lower Jaw		
		Inner Incisors	Outer Incisors	Canines	First Molars	Second Molars	Canines	First Molars	Second Molars
7	25	7	18	24	22	23	20	19	19
8	44	7	16	31	33	36	24	30	29
9	38	..	3	23	17	24	12	15	17
10	74	..	10	31	19	47	18	28	30
11	65	..	2	16	11	27	9	7	21
12	56	..	..	7	3	10	2	4	10
13	81	..	2	8	3	9	2	4	11
14	84	..	1	..	..	1	..	..	1
15	60	..	..	..	..	..	..	..	..

of deciduous and permanent teeth present among a total of all possible cases of teeth of each kind: teeth, not individuals, are the units. Permanent teeth protruding beyond the gums to any degree

[1] Boas, Franz., "Changes in Bodily Form of Descendants of Immigrants," *Reports of the Immigration Commission* (Washington, 1911), pp. 38, 129–137.

[2] Deaver, G. Gilbert, "Strength and Lung Capacity in Relation to Pubescence," MS. International Young Men's Christian Association College, Springfield, Mass., 1912.

[3] Crampton, pp. 351–355.

are counted as erupted teeth. Any recognizable fragment of a deciduous tooth was counted. Recognition is difficult in plaster casts and mechanical causes operate more freely on the fragments than on permanent teeth, hence, the tables for deciduous teeth are not so smooth as for the permanent.

TABLE II

PERCENTAGES OF DECIDUOUS TEETH PRESENT

Age	Upper Jaw					Lower Jaw		
	Inner Incisors	Outer Incisors	Canines	First Molars	Second Molars	Canines	First Molars	Second Molars
7	29	70	96	89	93	80	77	77
8	16	37	70	76	82	55	67	65
9	..	9	61	46	63	32	40	45
10	..	14	42	26	63	24	38	41
11	..	3	24	17	42	14	11	33
12	..	..	13	5	18	4	7	18
13	..	2	10	4	11	2	5	13
14	..	1	..	..	1	..	..	1
15	..	..	..	..	..	..	..	..

TABLE III

NUMBER OF PERMANENT TEETH PRESENT

Age	Cases	Upper Jaw							Lower Jaw				
		Inner Incisors	Outer Incisors	Canines	First Bicuspids	Second Bicuspids	First Molars	Second Molars	Canines	First Bicuspids	Second Bicuspids	First Molars	Second Molars
7	23	9	3	..	1	..	21	..	4	..	..	19	..
8	44	37	15	9	12	4	44	..	16	5	1	43	..
9	38	38	31	11	21	10	38	3	19	15	8	36	5
10	71	71	57	29	46	23	71	9	50	35	21	70	12
11	65	65	62	40	54	36	65	23	49	51	38	65	31
12	54	54	54	42	51	43	54	34	48	44	38	54	42
13	80	80	80	71	76	70	80	64	78	76	67	80	63
14	84	84	84	81	84	83	84	80	83	83	82	84	80
15	56	56	56	56	56	56	56	53	56	56	52	56	50
16	33	33	33	33	33	32	33	33	33	33	33	33	31
17	23	23	23	23	23	23	23	22	23	23	23	23	23
18	21	21	21	21	21	21	21	20	21	21	21	21	21
19	11	11	11	11	11	11	11	11	11	11	11	11	11

These give, for the loss of the deciduous teeth and the eruption of the permanent teeth, the following average ages and variabilities:

TABLE IV

PERCENTAGES OF PERMANENT TEETH PRESENT

Age	Upper Jaw							Lower Jaw				
	Inner Incisors	Outer Incisors	Canines	First Bicuspids	Second Bicuspids	First Molars	Second Molars	Canines	First Bicuspids	Second Bicuspids	First Molars	Second Molars
7	46	13	..	4	..	93	..	16	..	..	84	..
8	84	33	20	27	8	100	..	36	11	2	98	..
9	100	82	30	54	28	..	9	50	40	20	95	12
10	..	80	41	65	32	..	12	70	49	29	99	17
11	..	96	62	83	56	..	35	75	79	58	100	47
12	..	100	77	94	79	..	63	88	81	70	..	77
13	..	..	89	95	87	..	80	98	95	84	..	79
14	..	..	97	100	99	..	95	99	99	98	..	95
15	..	..	100	..	100	..	95	100	100	93	..	90
16	..	..	..	..	97	..	100	..	..	100	..	94
17	..	..	..	..	100	..	97	..	..	..	..	100
18	..	..	..	..	..	..	96	..	..	..	..	..
19	..	..	..	..	..	..	100	..	..	..	..	..

TABLE V

LOSS OF THE DECIDUOUS TEETH[1]

	Outer Incisors	Canines	First Molars	Second Molars
Upper	7.8 ± 1.6	9.5 ± 2.5	9.1 ± 1.9	10.3 ± 2.1
Lower	.....	8.4 ± 2.2	8.9 ± 2.2	9.2 ± 3.2

TABLE VI

ERUPTION OF THE PERMANENT TEETH

	Inner Incisors	Outer Incisors	Canines	First Bicuspids	Second Bicuspids	Second Molars
Upper	.....	8.5 ± 1.4	10.3 ± 2.2	9.2 ± 2.0[2]	10.7 ± 2.0[2]	12.1 ± 1.7
Lower	6.2 ± 1.5	7.0 ± 1.3	9.0 ± 2.3	9.9 ± 1.8	10.9 ± 2.0	11.4 ± 2.0

[1] Anterior-posterior lengths of permanent and deciduous upper teeth were incidentally obtained:

Permanent inner incisor	8.6 ± 0.7 mm.	618 cases.
" outer "	6.5 ± 0.7	547
" first bicuspids	6.7 ± 0.5	158
" second "	6.4 ± 0.5	161
Deciduous inner incisor	6.1 ± 0.4	16
" outer "	5.1 ± 0.7	50

[2] Upper bicuspids appear earlier than the corresponding teeth of the lower jaw in a mixed series of 4,850 boys and girls collected by James and Pitts, *Proceedings Royal Society of Medicine*, vol. 5 (London, 1912), pp. 80–101, and in a similar series of about 4,000 boys and girls by Livy, *British Medical Journal*, vol. II (London, 1885), p. 241.

For comparison, we have average ages and variabilities for the eruption of permanent teeth of the upper jaw based on plaster casts collected by Dr. Walter Channing (probably of Boston school children). These averages[1] are based on the number of children having permanent teeth among the total number of children, *e. g.*, a child with one or both permanent canines was counted as a child with permanent canines. Individuals, not teeth, are the units.[2] The averages are not strictly comparable, since by this method slightly earlier average ages are obtained than by using the teeth as units. We have also separated first and second bicuspids on the same casts.

TABLE VII

NUMBER OF UPPER BICUSPIDS PRESENT

Age	Boys			Girls		
	Cases	First Bicuspids	Second Bicuspids	Cases	First Bicuspids	Second Bicuspids
6	46	1	..	92	4	2
7	60	1	..	76	12	3
8	56	12	6	60	19	9
9	84	33	16	82	41	21
10	62	39	20	65	41	22
11	46	35	25	56	36	29
12	40	32	22	44	36	26
13	24	21	16	34	32	25
14	26	25	20	20	18	16
15	..	..	..	14	14	14

TABLE VIII

PERCENTAGES OF UPPER BICUSPIDS PRESENT

Age	Boys		Girls	
	First Bicuspids	Second Bicuspids	First Bicuspids	Second Bicuspids
6	2	..	4	2
7	2	..	16	4
8	21	11	32	15
9	39	19	50	26
10	63	32	63	34
11	76	54	64	52
12	80	55	82	59
13	87	67	94	74
14	96	77	90	80
15	..	..	100	100

[1] *Statistics of Growth*, p. 34.

[2] Information from Dr. Clark Wissler.

The average ages and variabilities for the Channing Series are as follows:

TABLE IX

	Inner Incisors	Outer Incisors	Canines	First Bicuspids	Second Bicuspids	Second Molars
Boys	7.5 ± 1.4	9.5 ± 2.1	11.2 ± 1.4	9.4 ± 2.2	11.4 ± 2.9	13.2 ± 2.0
Girls	7.0 ± 1.6	8.9 ± 2.1	11.3 ± 1.0	9.5 ± 2.5	11.2 ± 2.9	12.8 ± 1.6

As these figures stand, the Porto Rican boys appear to erupt their teeth as much as a year in advance of the Boston boys. However, the meaning of this difference is obscure, since Channing and Wissler found that a group of feeble-minded children erupted their canines and second molars a corresponding period in advance of the same Boston boys.[1]

TABLE X

	Boys	Girls
Canines	10.7 ± 1.4	10.0 ± 1.7
Second molars	10.9 ± 1.9	11.8 ± 0.9

If there is a correlation between physiological status and body measurements, then, at a certain age, those individuals accelerated beyond the average dental stage of their age should show a greater average stature than the remaining individuals. For example, eight-year-old boys with permanent upper canines are more developed than their age-mates who lack these teeth; as accelerated individuals, the group with canines should have an average stature greater than that of the group without canines. Similarly, a group lacking deciduous canines should be taller than a group of the same age with these teeth.

In tables XI and XII are given the average statures and variabilities of boys separated on the basis of the presence and absence of the deciduous teeth and the absence and presence of the permanent teeth, tooth by tooth.

[1] Channing, Walter, and Wissler, Clark: "The Hard Palate in Normal and Feeble-Minded Individuals," *Anthropological Papers, American Museum of Natural History*, vol. I (New York, 1908), pp. 316–319.

TABLE XI

Stature of Boys with Deciduous Teeth Present and Absent

(Statures in millimeters)

Upper Jaw

Age	Outer Incisors		Canines		First Molars		Second Molars	
	Present	Absent	Present	Absent	Present	Absent	Present	Absent
7	1141 ± 48	1139 ± 69	.....	.....	.....	.....	.....	.....
8	1166 ± 35	1189 ± 61	1182 ± 53	1173 ± 57	1177 ± 41	1187 ± 85	1181 ± 49	1170 ± 74
9	.....	.....	1245 ± 40	1268 ± 48	1243 ± 39	1263 ± 47	1241 ± 44	1276 ± 36
10	1266 ± 103	1276 ± 60	1242 ± 59	1299 ± 65	1247 ± 40	1284 ± 72	1274 ± 63	1275 ± 75
11	.....	.....	1263 ± 47	1325 ± 53	1277 ± 74	1317 ± 48	1296 ± 65	1321 ± 50
12	.....	.....	1336 ± 66	1389 ± 75	.....	.....	1356 ± 86	1386 ± 72
13	.....	.....	1308 ± 84	1394 ± 77	.....	.....	1294 ± 109	1397 ± 71

Lower Jaw

8	1195 ± 57	1155 ± 44	1195 ± 57	1155 ± 44	1182 ± 56	1168 ± 48	1178 ± 58	1176 ± 47
9	1241 ± 48	1260 ± 48	1241 ± 48	1260 ± 48	1241 ± 32	1261 ± 53	1252 ± 44	1253 ± 49
10	.....	.....	1253 ± 50	1281 ± 72	1242 ± 54	1294 ± 69	1260 ± 71	1284 ± 62
11	.....	.....	1235 ± 43	1317 ± 51	1265 ± 75	1311 ± 53	1281 ± 61	1319 ± 50
12	.....	.....	.....	.....	.....	.....	1383 ± 83	1382 ± 78
13	.....	.....	.....	.....	.....	.....	1307 ± 102	1398 ± 270

TABLE XII

Stature of Boys with Permanent Teeth Absent and Present

Upper Jaw

Age	Outer Incisors		Canines		First Bicuspids		Second Bicuspids		Second Molars	
	Absent	Present	Absent	Present	Absent	Present	Absent	Present	Absent	Present
8	1165 ± 41	1213 ± 64	1188 ± 52	1145 ± 49	1176 ± 41	1186 ± 81	1181 ± 45	1157 ± 95	.....	.....
9	1249 ± 23	1254 ± 48	1257 ± 42	1260 ± 47	1243 ± 39	1263 ± 47	1242 ± 41	1284 ± 36	.....	.....
10	1264 ± 78	1279 ± 63	1251 ± 58	1308 ± 68	1248 ± 57	1289 ± 69	1270 ± 48	1285 ± 71	1269 ± 61	1342 ± 69
11	.....	.....	1284 ± 51	1326 ± 57	1280 ± 80	1317 ± 51	1297 ± 65	1320 ± 51	1305 ± 60	1321 ± 53
12	.....	.....	1341 ± 60	1409 ± 64	.....	.....	1356 ± 86	1386 ± 72	1352 ± 62	1397 ± 77
13	.....	.....	1317 ± 82	1394 ± 77	.....	.....	1334 ± 134	1395 ± 67	1338 ± 103	1400 ± 68

Lower Jaw

7	1109 ± 26	1174 ± 60	.....	.....	.....	.....	.....	.....	.....	.....
8	1173 ± 40	1179 ± 57	1187 ± 57	1159 ± 48	.....	.....	.....	.....	.....	.....
9	.....	.....	1245 ± 50	1264 ± 45	1238 ± 36	1267 ± 51	1249 ± 41	1265 ± 50	.....	.....
10	.....	.....	1248 ± 48	1285 ± 73	1249 ± 57	1293 ± 70	1262 ± 62	1305 ± 76	1266 ± 56	1335 ± 70
11	.....	.....	1266 ± 55	1317 ± 52	1292 ± 65	1310 ± 54	1287 ± 61	1321 ± 49	1292 ± 58	1323 ± 51
12	.....	.....	1362 ± 74	1385 ± 79	1353 ± 81	1387 ± 78	1352 ± 73	1392 ± 78	1335 ± 76	1394 ± 75
13	.....	.....	.....	.....	.....	.....	1314 ± 96	1400 ± 70	1343 ± 79	1396 ± 80

TABLE XIII

Stature of Boys on the Basis of All Teeth

Age	Upper Jaw						Lower Jaw						Upper and Lower Jaws	
	Permanent		Deciduous		Permanent and Deciduous		Permanent		Deciduous		Permanent and Deciduous		Permanent and Deciduous	
	Absent	Present	Present	Absent	P. Absent D. Present	P. Present D. Absent	Absent	Present	Present	Absent	P. Absent D. Present	P. Present D. Absent	P Absent D. Present	P Present D. Absent
7	1142 ± 51	1147 ±61	1146 ±52	1134 ±56	1144 ± 51	1141 ±59	1138 ±52	1154 ±55	1144 ±55	1143 ±54	1140 ±53	1149 ±54	1142 ±52	1146 ±56
8	1178 ± 47	1181 ±60	1177 ±46	1182 ±61	1177 ± 46	1182 ±60	1181 ±51	1174 ±54	1182 ±55	1175 ±51	1181 ±52	1175 ±53	1179 ±49	1178 ±56
9	1249 ± 40	1259 ±46	1243 ±40	1260 ±45	1247 ± 40	1260 ±45	1248 ±42	1256 ±45	1249 ±43	1258 ±46	1249 ±42	1257 ±45	1247 ±41	1259 ±45
10	1262 ± 63	1283 ±68	1259 ±64	1280 ±68	1261 ± 63	1282 ±68	1259 ±57	1282 ±69	1252 ±60	1280 ±68	1257 ±58	1281 ±69	1259 ±61	1282 ±68
11	1294 ± 61	1316 ±55	1281 ±63	1317 ±54	1290 ± 62	1316 ±54	1286 ±59	1313 ±55	1267 ±61	1313 ±54	1281 ±60	1313 ±54	1286 ±61	1315 ±54
12	1353 ± 69	1387 ±73	1357 ±71	1383 ±73	1354 ± 69	1385 ±73	1349 ±73	1385 ±75	1319 ±74	1382 ±74	1341 ±74	1384 ±74	1348 ±71	1384 ±74
13	1324 ±105	1391 ±76	1308 ±93	1396 ±75	1319 ±100	1393 ±76	1314 ±88	1390 ±78	1251 ±45	1399 ±74	1292 ±80	1394 ±76	1303 ±90	1394 ±76

These data, together with a few cases referring to teeth not tabulated, are averaged below for corresponding values for the upper and lower jaws separately and finally together (table XIII). This is in effect simply weighting the stature of each individual according to his dental pattern.

After the ninth year the average statures of the two groups differ, the absolute difference increasing to the fourteenth year (see particularly the last two columns). These values show that there is a functional relation between stature and physiological status as defined by dentition.

For comparison, the upper bicuspids of boys and girls of the Channing series have been treated in the same manner with regard to stature and weight (tables XIV and XV).

TABLE XIV

STATURE OF CHILDREN WITH UPPER BICUSPIDS ABSENT AND PRESENT

Age	Boys				Girls			
	First Bicuspids		Second Bicuspids		First Bicuspids		Second Bicuspids	
	Absent	Present	Absent	Present	Absent	Present	Absent	Present
7	.....	.....	.....	.....	1162±44	1200±46	.....	.....
8	1229±47	1244±73	1232± 53	1231± 60	1216±44	1216±43	1217± 44	1214± 44
9	1267±59	1282±54	1297± 66	1286± 47	1250±55	1292±84	1264± 61	1292±107
10	1334±68	1319±62	1310± 59	1339± 65	1296+67	1318±64	1311± 68	1312± 63
11	1373±73	1319±91	1348± 69	1319±103	1323+68	1351±75	1329± 80	1351± 67
12	1374±74	1399±84	1381± 83	1405± 86	1335±56	1423±73	1374± 80	1430± 67
13	.....	.....	1438±122	1433± 89	.....	.....	1448±118	1527± 56

This smaller series gives affirmative results for both statures and weights.

TABLE XV

WEIGHT OF CHILDREN WITH UPPER BICUSPIDS ABSENT AND PRESENT
(Weight in Pounds Avoirdupois)

Age	Boys				Girls			
	First Bicuspids		Second Bicuspids		First Bicuspids		Second Bicuspids	
	Absent	Present	Absent	Present	Absent	Present	Absent	Present
7	.....	.....	.....	.....	54.7± 7.1	58.2± 6.0	.....	.....
8	54.8± 7.4	58.7± 5.2	55.3± 7.2	57.8± 6.2	56.0± 7.7	54.4± 5.2	56.1± 7.1	52.3± 4.6
9	60.0± 9.6	58.8± 7.6	57.7±10.3	60.8± 8.2	54.9± 5.9	60.2±10.0	56.1± 7.2	61.5±11.1
10	61.9± 6.9	62.6± 7.5	60.8± 6.4	65.7± 7.9	62.0± 8.8	63.2± 8.9	62.9± 9.1	61.4± 8.5
11	68.8±14.3	68.6± 7.3	66.2±11.3	70.8± 6.6	65.0± 8.1	64.7± 7.7	64.6± 7.9	65.1± 8.1
12	66.4±11.7	71.5±12.4	67.7± 3.0	73.2±12.3	65.2±10.9	77.3±12.7	73.2±16.1	76.0±10.8
13	.....	.....	76.3± 9.1	79.1±18.4	.....	.....	89.3±22.3	89.5±20.8

Having shown that variations in stature correspond to variations in the period of physiological development, we can obtain an insight into the character of the variations in period by considering each measurement as corresponding to the age to which it would belong according to the table of average statures for all boys (table XVI).

TABLE XVI

STATURES AND WEIGHTS OF ALL BOYS AND GIRLS

Age	Porto Rican Boys		Channing Series					
			Boys			Girls		
	Cases	Stature	Cases	Stature	Weight	Cases	Stature	Weight
6	..	.....	23	1131±38	52.1± 8.5	46	1126±43	51.3± 6.5
7	14	1144± 56	30	1165±60	53.6± 7.5	39	1168±45	55.3± 7.0
8	26	1179± 53	27	1232±53	55.6± 7.0	31	1216±43	55.5± 7.0
9	23	1254± 45	40	1273±57	59.5± 8.7	42	1271±73	57.5± 8.6
10	39	1275± 68	33	1323±63	62.3± 7.3	34	1310±65	62.8± 8.8
11	37	1311± 57	21	1332±89	68.7± 9.2	30	1340±73	64.9± 7.9
12	30	1381± 75	21	1394±81	70.7±12.3	22	1407±77	75.1±13.0
13	42	1386± 82	12	1434±98	78.2±15.8	16	1506±97	89.5±20.8
14	47	1468± 90	12	1552±85	88.1± 3.0	9	1541±99	99.9±30.7
15	32	1519±102	..	.....	.....	7	1599±53	108.0± 4.1
16	18	1571± 66	..	.....	.....	..	.....	.....
17	14	1602± 62	..	.....	.....	..	.....	.....
18	12	1627± 87	..	.....	.....	..	.....	.....
19	6	1620± 45	..	.....	.....	..	.....	.....

Confining our attention to the final values for Porto Rican boys (last two columns of table XIII), we can interpolate in this table for example for 1303 for stature and find 10.8 as the age for the corresponding average stature.

TABLE XVII

Age	Permanent Absent Deciduous Present	Permanent Present Deciduous Absent
7	7.0	7.0
8	8.0	8.0
9	8.9	9.3
10	9.3	10.2
11	10.3	11.2
12	11.5	12.7
13	10.8	13.1

These ages obtained by dentition and stature probably differ more from the corresponding chronological ages than would ages obtained on the basis of stature alone.

At the beginning of the period of rapid increase in the rate of growth, the accelerated individuals must be growing at a higher rate than the retarded individuals of the same chronological age but who have not yet entered into the period of rapid growth. Conversely, at the end of the period of rapid growth, the accelerated individuals must grow at a slower rate than the retarded. Consequently, at the beginning of the period, accelerated individuals must vary more than retarded individuals from the average of their age and less at the end of the same period. We have compared below the variabilities of the retarded and accelerated groups of each age.

TABLE XVIII

Age	Retarded Group	Accelerated Group	Difference
7	52	56	+ 4
8	49	56	+ 7
9	41	45	+ 4
10	61	68	+ 7
11	61	54	− 7
12	71	74	+ 3
13	90	76	−14

While the results are not marked, the accelerated group varies more than the retarded group at the beginning of the period of rapid increase of the rate of growth, but less at the end of the same period. The converse holds true for the retarded group. From another point of view the increased variabilities in stature for both accelerated and retarded groups may be an expression of a different rate of development from that of dentition. If this is so, the greater the degree of acceleration or retardation of dental development, the greater will be the variations in stature.[1]

These data show that there is a marked functional relation between stature and stage of dental development. Variations in stature, aside from such as are due to heredity and perhaps to other causes, are then expressions of variations in the period at which certain stages of development are reached.

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NEW YORK CITY

[1] Boas, Franz, "The Growth of Children," *Science*, N. S., vol. XXXVI, no. 937, p. 817.

ON COMPUTATIONS FOR THE MAYA CALENDAR

By RAYMOND K. MORLEY

IN *A Method which may have been used by the Mayas in Calculating Time*, and also in *The Numeration, Calendar Systems, and Astronomical Knowledge of the Mayas*, Appendix VII, Mr. C. P. Bowditch gave a rule for shortening the computations for finding a terminal date in the Maya calendar count when the initial date and the interval of time elapsed from it are given. This rule has perhaps not had the use it deserves, partly because of the natural prejudice of modern scholars in favor of expressing numbers in the familiar decimal system, and partly because the rule as given by Mr. Bowditch is still a little cumbrous. It is the purpose of the present paper, first, to develop modifications of Mr. Bowditch's method which make it more easily used, and second, to give a new formula for the solution of the reverse problem, namely, given two dates to find the interval of time between them. These deductions will be prefaced by some general considerations concerning computations in the Maya system of numeration. Their system, so far as we know it, was a curious mixture. The bar and dot notation is quinary, the face numerals from 13 (or sometimes 14) to 19 and the corresponding native names for these numbers are decimal, and the calendar system as a whole is vigesimal, with the exception of the step from uinals to tuns. Irregular though the system is, it is the author's belief that the greatest facility in handling Maya time periods will come by keeping them expressed so far as possible in the Maya fashion.

Some operations present no difficulty in an irregular system like the Maya. We handle repeatedly somewhat similar notations. For instance 4 hours 3 minutes 6 seconds; £12 13 s. 6 d.; 78° 24′ 30″ and so on. There is no difficulty in performing simple operations on these. Adding them, subtracting them, multiplying or dividing them by small numbers are all quite easy, the only pre-

caution to be observed is the reduction of one kind of unit to the next in carrying or borrowing. So with the Maya system of time periods. For instance:

	17. 7. 6.12. 1		3. 1.14. 6. 8
+	4.15. 7.10.13	−	1. 2. 6.17. 9
	1. 2. 2.14. 4.14		1.19. 7. 6.19

The only point requiring special care here is to note that the amount carried from uinals to tuns or borrowed from tuns to uinals is 18. Again as an example of multiplication by a small number let us find 11 times 2.12.13. 0.

$$\begin{array}{r} 2.12.13.0 \\ \times \quad 11 \\ \hline 1.\ 8.19.17.0 \end{array}$$

The steps are: $11 \times 0 = 0$. $11 \times 13 = 143$ which is (dividing by 18) 7 tuns and 17 uinals. Write the 17 uinals and carry 7 tuns. $11 \times 12 = 132$ and 7 to carry is 139 which is (dividing by 20) 19 and 6 to carry. $11 \times 2 = 22$ and the 6 makes 28, which is 8 and 1 to carry. Write the 1. Short division goes in a very similar way: to divide 14. 4.17. 3. 2. by 5:

$$\begin{array}{r} 5)\ 14.\ 4.17.\ 3.\ 2 \\ \hline 2.16.19.\ 7.16 \text{ and a remainder of } 2. \end{array}$$

The steps are: 5 into 14 twice and 4 over; the 4 over is 4 twenties or 80 of the next grade, a total of 84 of the next grade; 5 into 84 16 times and 4 over, which makes a total of 97 of the next grade; 5 into 97 19 times and 2 over. 2×18 is 36 of the next grade and 3 makes a total of 39. 5 into 39 7 times and 4 over, 82 of the next grade. 5 into 82 16 times and a remainder of 2.

However, in the case of a multiplication by a larger number, so that handling it would require the use of partial products, our ordinary process breaks down. Suppose we try to multiply in the usual way, by means of partial products, 2.12.13.0 by 73. We can multiply by the 3 at once

$$\begin{array}{r} 2.12.13.0 \\ \times \quad 3 \\ \hline 7.18.\ 3.0 \end{array}$$

but the next partial product cannot be formed by multiplying by 7 and setting over one place, because the actual step is multiplying by 70 and the setting over in the Maya system is not equivalent to multiplying by 10 but by 20 in most cases, but to multiplying by 18 when setting across the 18 step from uinals to tuns. If it were not for this step, that is, if the Mayas used a purely vigesimal system, writing the multiplier also in the same system would solve the difficulty and allow us to apply our ordinary processes at once. A long division involves fully as many difficulties for a similar reason. This does not mean that these processes could not be carried out by some means, but that it is impractical because our usual rules do not apply to the operations. Fortunately, however, the calculations ordinarily required for the Maya calendar are of a very restricted variety, and by the use of special methods for these problems nearly all time units may be kept in the Maya system, avoiding the tedious and rather useless reduction to the decimal notation that has proven the popular method with most investigators (*e. g.*, Förstemann, Seler, S. G. Morley).

There are two principal problems that we have to solve in connection with the calendar: (1) Given a starting date by day coefficient, day name, month coefficient, and month name, and a period of time counted from it, required the terminal date by coefficients and names. (2) Given the two dates, required the distance from one to the other. Let us take them up in order.

As is explained in treatises on the Maya calendar finding one date a certain distance from another is unaffected by diminishing the distance by any number of complete calendar rounds of 52 haab or years. To subtract the greatest possible number of complete rounds diminishes the subsequent work somewhat.[1] 1 haab = 1.0.5. Multiply this by 52 as follows: 52×5 kins = 260 kins or 13 uinals; 52×1 tun = 52 tuns = 2 katuns and 12 tuns. Hence $1.0.5 \times 52 = 2.12.13.0$. That is 1 calendar round = 2.12.13.0. For short intervals, say less than 10 rounds, it is easy to multiply this at once for the purpose of getting the multiple to

[1] This process of diminishing the distance by calendar rounds, while the usual method, can be avoided, as will be shown later.

be subtracted. *E. g.*, 7 rounds = 7 × 2.12.13.0 = 18.9.1.0. The process is: 7 × 0 = 0, 7 × 13 = 91 = 18 × 5 + 1, write 1 and carry 5; 7 × 9 = 84, and 5 to carry is 89 which = 4 × 20 + 9, write 9 and carry 4; 7 × 2 = 14, add 4 = 18. But for long intervals such as are found in the Initial Series this is too difficult. A table is therefore desirable. Such a table may be found in Morley, *Bureau of American Ethnology*, Bulletin 57, p. 144. The column headed Cycles, etc., is the one to use. When all complete calendar rounds have been subtracted the distance number to be treated is always less than 2.12.13.0.

The problem of finding the terminal date consists of three parts: (1) Counting from the given day name a number equal to the remainder on dividing the distance by 20 to find the new day name; (2) Counting from the day coefficient a number equal to the remainder on dividing the distance by 13; (3) Counting from the day in the year a number equal to the remainder on dividing the distance by 365 days or 1.0.5.

1. There is no difficulty with the first of these. The required number is exactly the kin number of the given distance, since 20 kins = 1 uinal, and the tuns, etc., are all exact multiples of the uinal. To get the day name count forward (backward sometimes when the count as a whole is backward. I shall hereafter assume that it is forward. The methods for finding the remainders apply) a number equal to the kin number.

2. The remainder on dividing by 13 might be found by the process indicated above for short division, but the following modification of the method of Mr. Bowditch already referred to is practically much more convenient. To understand it we shall first develop some general principles.

A. The remainder on dividing a number by a divisor is not affected by adding to or subtracting from the number any exact multiple of the divisor. For, regard the process of dividing as counting around and around in a circle composed of as many places as there are units in the divisor (of course it is from just such circular counting in the Maya calendar that the need for the remainder arises). Then it is clear that to add or subtract an

exact multiple of the divisor will change the number of complete rounds but will not affect at all the amount left over. If a number be diminished by the next less exact multiple of the divisor, that is if the number be replaced by the remainder on dividing by the divisor, the process is frequently referred to as "casting out" the divisor. Thus, casting out 13 from 68 gives 3; casting out 13 from 97 gives 6, etc.

B. If a number divided by a divisor gives a certain remainder, a multiple n times the number will have a remainder n times as great as the original remainder, or will differ from it by a multiple of the divisor. Thus, 12/9 gives a remainder 3, hence 2×12 divided by 9 gives a remainder $2 \times 3 = 6$. Again, 5×12 divided by 9 gives a remainder 6 which $= 5 \times 3 - 9$. That this must be the case will appear from considering again the counting around a circle. For if the number once counted around and around ends a certain distance (*i. e.*, the remainder) beyond the starting point, counted twice it will end as much further beyond the new starting point, *i. e.*, twice as far from the first starting point, and so on. If these together make up more than a complete revolution it will be necessary to deduct a multiple of the divisor. Further examples follow: 20/13 leaves a remainder of 7; therefore 4×20, that is 80, leaves a remainder of $7 \times 4 - 2 \times 13 = 2$ as may readily be verified. $360 = 20 \times 18$. Hence 360/13 gives remainder of $7 \times 18 = 126$ less 117 (13×9) or 9. This last is more easily done by the aid of Principle *A*, thus: $20 \times 18 = 20 \times 13 + 20 \times 5$. Casting out 20×13 (Prin. *A*) we may treat 20×5 instead. $5 \times 7 = 35$; casting out 13, $35 - 26 = 9$ as before. In using Principle *B*, therefore it is never necessary to use a multiplier larger than the divisor, in these cases 13. If one such occurs the remainder is unaltered by casting out 13 from it at once.

We will now apply these principles to finding the remainder on dividing 1 uinal, 1 tun, 1 katun, and 1 cycle (the cycle is unnecessary if all complete calendar rounds have been subtracted) respectively by 13.

1 uinal divided by 13 remainder 7.

1 tun = 18 uinals which will have same remainder as $18 - 13 = 5$ then $7 \times 5 = 35$ cast out 13 remainder 9.

1 katun = 20 tuns, same remainder as 7 tuns; then $7 \times 9 = 63$ cast out 13 remainder 11.

1 cycle = 20 katuns, same remainder as 7 katuns, then $7 \times 11 = 77$ cast out 13 remainder 12.

To put these results together we need another principle, *C*. Namely that the sum of the remainders of several numbers each divided by a divisor, after casting out the divisor from this sum, is the same as the remainder on dividing the sum of the numbers themselves.

Another consideration of counting in a circle will show the truth of this.

If then we have a number expressed in cycles, katuns, uinals, and kins the total remainder is given by the following sum

kin number + uinal number × 7 (7 is remainder on dividing 1 uinal by 13, then apply Principle *B*),
+ tun number × 9 (9 is remainder from 1 tun as found on p. 53, then by Principle *B*),
+ katun number × 11 (11 is remainder from 1 katun as found above),
+ cycle number × 12 (12 is remainder from 1 cycle as found above).

In using this method 13 should be cast out at every possible stage. If the kin number exceeds 13, cast it out. If the uinal, tun, katun, or cycle number exceeds 13 cast it out before forming the product, and cast out 13 from each product before adding it to the preceding and whenever the sum exceeds 13 cast it out.

This rule is quite feasible, but a further diminution of the size of the numbers involved is possible. Consider again counting around the divisor circle. If the remainder is more than half the divisor we shall evidently arrive at the same point more quickly by counting backward the difference between the remainder and a complete revolution (*i. e.*, the divisor). This difference represents the amount the number being counted falls short of the next larger multiple of the divisor. If we are considering multiples of the number, or sums of several numbers, their backward remainders

may be multiplied and added like ordinary remainders and the amount to be counted ahead finally found by deducting their total from the divisor or from the next greater multiple of the divisor.

Now it will be noticed that the remainders from 1 uinal, 1 tun, 1 katun and 1 cycle after dividing by 13, as found on pages 53–54, are all more than half of 13. The backward remainders, $13 - 7 = 6$, $13 - 9 = 4$, $13 - 11 = 2$, and $13 - 12 = 1$ will give us smaller multipliers. This suggests the following rule: Multiply the uinal number (first casting out 13 if possible) by 6 and cast out 13; multiply the tun number (less 13 if possible) by 4 and cast out 13; add this to the preceding result and so on. Then subtract this sum from 13, add the kin number (less 13 if possible) and again cast out 13. The order of these additions and subtractions may be interchanged with advantage giving the following final form for the rule:

Rule 1,[1] for Finding the Remainder on Dividing by 13 a Period of Time Expressed in the Maya System

Multiply the cycle number (less 13 if possible) by 1 (that is, take it as it stands).

Multiply the katun number (less 13 if possible) by 2. Add to preceding and cast out 13.

Multiply the tun number (less 13 if possible) by 4. Add to preceding and cast out 13.

Multiply the uinal number (less 13 if possible) by 6. Add to preceding and cast out 13.

Subtract this result from the kin number increased by 13 if necessary to keep the difference positive. This will give the remainder on dividing by 13.

[1] Whether the reader has followed the preceding reasoning or not it will prove a great time saver if he is to do much work with the Maya calendar to memorize this and the following rules. Its advantage consists largely in the fact that the small size of the numbers permits of performing all the operations mentally, or writing only the previous sum to be carried along. We often use rules without being very clear as to their reasons. For instance many people who cast out 9 in our decimal system by adding the digits could not justify the process. As a matter of fact the principles involved are similar to those here used. The sequence of multipliers in this rule (beginning with the cycle multiplier and ending with that for the uinals) 1, 2, 4, 6 is an easy one to remember. It is a curious coincidence that they add up to 13.

As already pointed out it is not *necessary* to handle distances as great as cycles, because the cycle number may always be removed by subtracting calendar rounds, but it is so easy to include them in the rule that it seems desirable to do so.

Some examples will now be treated. Required the remainder on dividing 9.6.4.10.5 by 13. Perform the operations thus: $9 \times 1 = 9$, $6 \times 2 = 12$, and $9 = 21$, cast out $13 = 8$. $4 \times 4 = 16$, and $8 = 24$, cast out $13 = 11$. $10 \times 6 = 60$, and $11 = 71$, cast out $13 = 6$. 5, the kin number, is less than 6, so add 13. $13 + 5 = 18$, less $6 = 12$, the required remainder.

Again, 9.17.15.16.11. $9 \times 1 = 9$. $17 - 13 = 4$, $4 \times 2 = 8$, $8 + 9 = 17$, $17 - 13 = 4$. $15 - 13 = 2$, $2 \times 4 = 8$, $8 + 4 = 12$. $16 - 13 = 3$, $3 \times 6 = 18$, $18 + 12 = 30$, $30 - 26 = 4$. 11 (kin number) $- 4 = 7$, the required remainder.

It will be noticed that the largest possible product with this method is $6 \times 12 = 72$, and the greatest possible multiple of 13 necessary for casting out therefore is $5 \times 13 = 65$. To carry out the whole process mentally involves no more labor than an ordinary short division.

The remainder so found is to be counted in the usual manner from the given day coefficient to find the new day coefficient.

3. Finding the remainder after dividing by 365, or a haab, or 1.0.5 may be done in a very similar way, except that the grade numbers cannot be diminished as before. The inconvenient form of the cycle remainder makes it perhaps a little easier to suppose the cycle number removed by deducting calendar rounds as previously explained but it is not necessary. Proceeding as for 13, the remainder for 1 kin is 1 kin, the remainder for 1 uinal is 20 kins or 1 uinal, the remainder for 1 tun is 360 kins or 18 uinals, but the backward remainder is much smaller, namely 5 kins. The backward remainder for 1 katun is 20 times this, or 5 uinals. In precisely the same way as before, in the case of 13, we get the following:

The improvements of the rule given here over Mr. Bowditch's method consist in casting out the 13 at every stage, thus bringing the work within the bounds of mental computation, in avoiding the plus and minus signs, which are confusing to a mind not mathematically trained, and in arranging the process in a rule consisting of very definite steps always to be taken in the same way.

RULE 2, FOR FINDING THE REMAINDER ON DIVIDING BY 1.0.5 A PERIOD OF TIME EXPRESSED IN THE MAYA SYSTEM

Multiply the tun number by 5 and write it as kins, reducing to uinals and kins if result exceeds 20.

Multiply the katun number by 5 and write it as uinals, reducing to uinals and tuns if result exceeds 18.

Add these two, and subtract their sum from the uinals and kins of the given distance, adding a sufficient multiple of 1.0.5. to keep the difference positive. The result will be the required remainder expressed in uinals and kins. It should be counted in this form, the uinal number being exactly adapted to counting uinals without further reduction.

This method may be extended to include cycles, thus obviating the necessity of subtracting calendar rounds by means of a table. The student may choose between the following and the calendar round table. The backward remainder from 1 katun was seen to be 5 uinals. From one cycle it is then 20 × 5 uinals or 100 uinals = 5.10.0. Casting out 5 × 1.0.5. this becomes 0.8.15. The remainder itself is then 1.0.5 − 8.15 = 9.10. The following addition therefore to the Rule just given takes care of cycles.

Add to the previous result the cycle number times 9.10 casting out 1.0.5. from the result. As the majority of cycle numbers is 9 the amount to be added in most cases is 9 × 9.10 = 4.13.10 which on casting out 4 × 1.0.5 gives 12.10. To remember this last figure and add it will probably save time over using the calendar round table.

Appendix to Rule 2.—Multiply 9.10 by the cycle number, add the previous result and cast out 1.0.5. If the cycle number is 9 add 12.10 at once (casting out 1.0.5. if possible).

Some examples will now be considered. Required the remainder on dividing 9.6.4.10.5 by 1.0.5.

4 × 5 = 20 kins	= 1.0	Given uinals and kins	10. 5
6 × 5 = 30 uinals	= 1.12.0	Multiple of 1.0.5 to keep result positive	2. 0.10
Sum	1.13.0	Sum	2.10.15
		Deduct	1.13. 0
			15.15
		Add 12.10 for the 9 cycles	12.10
		Sum	1.10. 5
		Cast out 1.0.5 for the required result	10. 0

The step marked with the brace would have been unnecessary if calendar rounds had been deducted at the start. In following the present method a change of order is perhaps better, as in the next example.

	9.17.15.16.11		
15 × 5 = 75 kins	= 3.15	Uinals and kins given	16.11
17 × 5 = 85 uinals	= 4.13. 0	Remainder for 9 cycles	12.10
Sum	4.16.15	Add 4 × 1.0.5	4. 1. 0
		Sum	5.12. 1
		Deduct	4.16.15
		Required remainder	13. 6

Next let us apply these methods of finding remainders to a case from the Maya calendar. Required the terminal date a distance of 9.12.8.14.1 forward from 4 ahau 8 cumhu. The remainder on dividing by 20 is the kin number 1. The day name is therefore 1 forward from ahau; that is, imix. For the remainder on dividing by 13, $9 \times 1 = 9$. $12 \times 2 = 24$, $24 + 9 = 33$, $33 - 26 = 7$. $8 \times 4 = 32$, $32 + 7 = 39$, $39 - 39 = 0$. $14 - 13 = 1$, $1 \times 6 = 6$. $6 + 0 = 6$. 1 (kin number) + 13 = 14, $14 - 6 = 8$. 8 days forward from 4 gives $4 + 8 = 12$. So far then 12 imix. For the remainder on dividing by 1.0.5.

8 × 5 = 40 kins	= 2.0	Uinals and kins given	14. 1
12 × 5 = 60 uinals	= 3.6.0	Remainder for 9 cycles	12.10
Sum	3.8.0	2 × 1.0.5 to make sum exceed 3.8.0	2. 0.10
		Sum	3. 9. 1
		Deduct	3. 8. 0
			1. 1

Count forward 1 uinal from 8 cumhu would be 8 pop, but we must deduct 5 kins as we pass uayeb, which gives 3 pop. Then 1 kin makes 4 pop. Result 12 imix 4 pop.

Again, let us reckon the terminal day reached by counting 9.0.19.2.4. from 4 ahau 8 cumhu. Counting forward the kin number, 4, from ahau the day name is evidently kan. Next find the remainder on dividing by 13. In writing the process the result of each step only will be written 9. 0, 9. $24 + 9 - 26 = 7$. $12 + 7 = 19$, *i. e.*, 6. 4 (kins) $+ 13 = 17$. $17 - 6 = 11$. 11 days forward from 4 is 2. So far 2 kan. $19 \times 5 = 95 = 4.15$.

Given	2. 4
9 cycles	12.10
	14.14
Deduct	4.15
Result	9.19

Count forward 9 uinals from 8 cumhu, deducting 5 kins as you pass uayeb from the 19 kins, result 8 chen and 14 kins yet to go, which brings 2 yax. The whole result then is 2 kan 2 yax.

The Second Problem

Take up now the second problem stated above, namely, given two dates by names and coefficients, required the distance from the first to the second. It is evident that except for the fact that all complete calendar rounds must be thought of as rejected this problem is the reverse of the other. That is we must now find a distance such that the remainder on dividing by 20 (*i. e.*, the kin number) shall be what we get by counting from the first given day name to the other day name, and also such that the remainder on dividing by 13 shall be what we get by counting from the first given day coefficient to the other day coefficient, and also such that the remainder on dividing by 1.0.5. or 365 kins shall be what we get by counting from the first day in the year to the second. It will be convenient to use the following abbreviations:

For the count from the first day name to the second, X;
" " " " " " " coefficient to the second, Y;
" " " " " " " in the year to the second, Z.

Stated in terms of these symbols the problem is to find a distance such that the remainder on dividing by 20 (*i. e.*, the kin number) is X, on dividing by 13 is Y, and on dividing by 365 or 1.0.5 is Z. Let us try to satisfy the last two conditions first. If the distance divided by 13 leaves a remainder Y it must be possible to write it in the form $13m + Y$, where m is a whole number (the quotient). Similarly it can be written $365n + Z$, where n is a whole number. But these represent the same distance, hence $13m + Y = 365n + Z$ or dividing by 13

$$m = \frac{365n + Z - Y}{13}, \text{ which } = 28n + \frac{n + Z - Y}{13}.$$

Denote by $[Z - Y]_{13}$ the remainder on casting out 13 from $Z - Y$. Then the only part of this value of m which is not a whole number on the face of it is

$$\frac{n + [Z - Y]_{13}}{13}.$$

m however is a whole number, consequently the numerator of this must be divisible by 13. This will happen if $n + [Z - Y]_{13} = 13$ or 26 or 39 or 52, etc. Then n must be $13 - [Z - Y]_{13}$ or this number increased by a multiple of 13. The distance therefore is $365 \times \{13 - [Z - Y]_{13}\} + Z$ or this plus 365×13 or $365 \times 13 \times 2$ or $365 \times 13 \times 3$. It is not necessary to go further, for $365 \times 13 \times 4$ is a complete calendar round. What determines the proper multiple of 365×13 to add? Evidently the one remaining condition to be satisfied, that the kin number of the found distance shall equal X. Expressing distances now in the Maya notation the rule may be formulated thus:

Rule 3, for Finding the Distance between Two Given Dates

Compute $\{13 - [Z - Y]_{13}\} \times 1.0.5 + Z$,[1] where Y is the count from one day coefficient to the other, and Z is the count from one day in the year to the other, and $[Z - Y]_{13}$ signifies the remainder on casting out 13 from $Z - Y$. If this result has the correct kin

[1] The division by 13 in finding $[Z - Y]_{13}$ should be done in the Maya notation by the short division process explained on page 50.

number (*i. e.*, X, the count from the first day name to the second) it is the required distance. If not, subtract its kin number from the required one, X (borrowing 20 if necessary). The difference must be 5, 10, or 15.

If 5 (= 1 × 5) add 1 × 13 × 1.0.5 = 13.3.5
" 10 (= 2 × 5) " 2 × 13 × 1.0.5 = 26.6.10 } [1]
" 15 (= 3 × 5) " 3 × 13 × 1.0.5 = 39.9.15 }

This will give the required distance. If Z should be less than Y it may be increased by 1.0.5 in computing $[Z - Y]$ so as to keep this difference positive.

To facilitate the use of this method it will now be given in a less exact way which is perhaps easier to remember and which is enough to recall the successive steps.

RULE 3*a*. The Year Position Difference Less the Day Coefficient Difference. Cast out 13. Subtract from 13. Multiply by a year, and add to the year position difference. If this is not right add enough 13 years to make it right. Test correctness by comparing kin number with day name difference.

As an example of the application of this rule let us find the distance from 2 kan 2 yax to 7 muluc 17 tzec. Here X = count from kan to muluc = 5. $Y = 7 - 2 = 5$. Z = count from 2 yax to 17 tzec = 13 uinals + 17 − 2 kins + 5 kins in uayeb = 14.0. The formula to be computed is then $\{13 - [14.0 - 0.5]_{13}\} \times 1.0.5 + 14.0$. Now $14.0 - 0.5 = 13.15$. $[13.15]_{13} = [15]_{13} = 2$. $13 - 2 = 11$. $11 \times 1.0.5 = 11.2.15$.[2] $11.2.15 + 14.0 = 11.16.15$. But the day name difference, X, is 5 which does not agree with this 15 kin number. Now $5 + 20 - 15 = 10$, which is 2 times 5. Add therefore 2 times $13 \times 1.0.5 = 26.6.10$ and the result is $38.5.5 = 1.18.5.5$, which is the required distance.

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[1] Of course 26 tuns and 39 tuns should be reduced to katuns and tuns to adhere to the Maya notation, but it is probably easier to remember them as written here, by means of the progressions, 5, 10, 15 for kins; 3, 6, 9 for uinals; and 13, 26, 39 for tuns.

[2] Multiplication of this product should be carried out in the Maya notation as explained on page 50.

CERTAIN PRE-COLUMBIAN NOTICES OF THE INHABITANTS OF THE ATLANTIC ISLANDS

By W. H. BABCOCK

THIS paper does not pretend to be exhaustive. Territorially it is confined to the eastern oceanic islands, from Iceland to the Canaries.

Of the first we have exceptionally ample information as to the settlement by the Norsemen about the opening of the tenth century and the subsequent fortunes of that turbulent but brave and intellectual northern republic. We know also that a few Irish monks had preceded them along the coast. Nansen holds that there may have been an inland Celtic colonization as well; but the local names on which he relies may be accounted for better by the presence among the Norsemen of many Irishmen and Hebrideans, both captives and free immigrants, such as arrived in the train of Queen Aud the Deep Minded of Dublin.

There are some suggestions, however, in the thirteenth century sagas which may be treated either as the offspring of fancy or as the records of last remnants of aborigines, taking refuge in the interior fastnesses of the land. Thus the well-known saga of Grettir tells how that formidable outlaw in one crisis of his fortune dwelt for a season with a family of trolls in a hidden valley of a difficult and secluded mountain. The word troll was certainly used, before and after this time, in some instances to designate the Eskimo of Greenland. It thus occurs in the Floamanna saga and other writings. This raises the possibility that there were once Eskimo in Iceland. But there is no corroborative evidence, and the passages in question are (more likely) borrowings from Norwegian tradition or fancy flights of the romancer.

The Azores seem to have been occasionally visited very early indeed; even a hoard of Carthaginian and other northern African Phoenician coins having been found on the coast of Corvo, the

nearest island to America. There are various passages in old writers of various periods, as will be seen, which may indicate later visits, also the discovery of inhabitants, but these points remain very uncertain. Nowhere is archaeologic and anthropologic research more needed, especially in the two outlying members of the archipelago.

Perhaps the most widely discussed account of an Atlantic island is what Plato declared concerning Atlantis in the fifth century before Christ, on the faith of a much earlier poem and narrative by his ancestor Solon, who in turn derived his information, as alleged, from the priests of Sais in Egypt, who were the repositories of very ancient traditions. Judging by climate and the statement of location, Madeira or some land not very far removed might be meant, though certainly conceived of as on a much larger scale than anything now existing there. Many different explanations of the legend have been proposed. About the most recent of them finds in it a distorted, exaggerated, and transplanted history of the domination and downfall of the Minoan Sea Kings of Crete. Others have assumed it to be a mere creation of the romantic fancy of Plato himself. But this seems untenable; and the general drift of opinion is toward viewing the tale as a tradition repeated in good faith and located from the beginning in the Atlantic Ocean, whatever its foundation in fact.

The tale runs as follows, using a popular English translation with some elimination and condensation:

> These histories tell of a mighty power which was aggressing wantonly against the whole of Europe and Asia and to which your city put an end. This power came forth out of the Atlantic Ocean, for in those days the Atlantic was navigable, and there was an island situated in front of the straits which you call the Columns of Hercules; the island was larger than Libya and Asia put together and was the way to other islands, and from the island you might pass through the whole of the opposite continent which surrounded the true ocean. . . . Now in the island of Atlantis there was a great and wonderful empire, which had rule over the whole island and several others, as well as parts of the continent. . . . But afterward there occurred violent earthquakes and floods and in a single day and night of rain the island of Atlantis disappeared and was sunk beneath the sea. And that is the reason why the sea in those parts is impassable and impenetrable, because there is such a quantity of shallow mud in the way; and this was caused

by the subsidence of the island. . . . Because of the greatness of the empire many things were brought to them from foreign countries. There were a great number of elephants in the islands.

Numerous kinds of fruits, flowers, legumes and other vegetable productions are described as

brought forth fair and wondrous in infinite abundance. . . . They employed themselves in constructing their temples and palaces, harbors and docks, a great palace which they continued to ornament during many generations, canals and bridges, walls and towns, numerous statues of gold, fountains both cold and hot, baths and a great multitude of houses. In the mountains were many wealthy inhabited villages. The plain was rectangular and for the most part straight and oblong.

The inhabitants are represented as worshipping Poseidon, their first founder, and being ruled by his laws which were inscribed on a column erected in the center of the island.

The reference to difficulties of navigation may indicate acquaintance with the Sargasso sea of weeds beyond the Azores; but the distinctively human data belong rather to the most advanced regions of the eastern Mediterranean in Plato's time, and the elephants of Atlantis are certainly oriental, not Atlantic. On the whole we do not get the impression that Plato or Zeno or the Egyptian priests had any real information about the inhabitants of Atlantic islands.

In the first century before Christ Diodorus Siculus wrote in his Library what Brown's translation (in the headline of Chapter 2, Book 5) identifies as an account of "Madeira."

Over against Africa lies a very great island in the vast ocean, of many days sail from Lybia westward. The soil here is very fruitful, a great part whereof is mountainous but much likewise champaign, which is the most sweet and pleasant part of all the rest, for it is watered with several navigable rivers, beautified with many gardens of pleasure, planted with divers sorts of trees and abundance of orchards interlaced with currents of sweet water. The towns are adorned with stately buildings and banqueting houses up and down, pleasantly situated in their gardens and orchards. And here they recreate themselves in summer time as in places accommodated for pleasure and delight. . . .

The mountainous part of the country is dotted with many large woods and all manner of fruit trees. . . . There you may have game enough in hunting all sorts of wild beasts. The air and climate of the island is very healthful and

mild, so that the trees and fruit and other things that are produced there are fresh and beautiful most part of the year.

The Phœnicians . . . passed at length beyond the pillars of Hercules into the sea called the ocean and first they built a city called Gades near to Hercules' pillars and at the sea side of an isthmus of Europe. . . . The Phœnicians having found out the coasts beyond the pillars and sailed along the shores of Africa were on a sudden driven by a furious storm far off into the main ocean and after they had been under a violent tempest for many days they at length arrived at this island and thus coming to the knowledge of the nature and pleasantness of this isle they were the first that discovered it to others. . . . The Etrurians when they were masters of the sea designed to send a colony thither; but the Carthaginians opposed them, fearing that most of their own citizens should be allured by the goodness of the land to settle there and likewise intending to keep it as a place of refuge for themselves.

The climate, physical characteristics and geographical situation all seem to point to Madeira; but the human life reported has no note of authenticity. It seems to echo observations along the Mediterranean coasts of Europe.

The elder Pliny narrated not much later a Mauritanian naval expedition to the Fortunate Islands or Canaries, which met with no inhabitants, though they found a temple on an island which they called Junonia, perhaps the modern Gomera, with other necessary indications of human occupancy.

Plutarch describes an island, Ogygia, five days' sail from England and mentions a continent beyond; which statement may be reminiscent of America. But there is no certainty, and he seems to regard the inhabitants as transplanted Greeks, except the God Saturn, who lay asleep in one of the islands.

There are various other references to western lands in ancient authors and those of the dark ages and early middle ages, but these have little or no anthropologic value until we come to Edrisi.

This great Arabian geographer prepared from the reports of many investigators and from many books a world map in silver for King Robert of Sicily about 1155. He also wrote a full geographical account of the known world, illustrated by a world map, probably copied from the silver original and by many detail maps of zones called climates and divided by latitudes. Of this important work four manuscript copies are still extant, two of them being in

Paris and two in the Bodleian library in Oxford. I have obtained good rotograph copies of all the parts of maps illustrating Atlantic islands and belonging to these two Bodleian manuscripts. The divergency is great, and probably greater still from the Paris copies, to judge by the published restorations which have been attempted from time to time. Clearly no one is in a position to restore exactly what Edrisi originally showed.

There is a full but rather old translation into French by Jaubert from the two Paris manuscripts and another of 1866 by Dozy and Goeg'e of the part concerning Africa, Spain and the Atlantic islands from all four manuscripts. The latter has been usually followed by me in the quotations given below—with some slight condensation.

Edrisi tells us at the beginning of his account of the First Climate of two islands called the Fortunate Isles or El Khalidat; giving no human particulars excepting that a stone column had been found on each of them supporting a copper statue pointing to regions beyond. He reports that there was said to be six of these pedestals and statues, the first being at Cadiz. There was not known to be any inhabited land beyond. He adds that Ptolemy reckoned longitude and latitude from these islands. It may be that he had chiefly in mind Lanzarota and Fuerteventura, the two most easily accessible members of the Canary group, confusing them a little with Ferro, another island of the same archipelago farther at sea.

Later, in treating of the Third Climate, which by his system would be farther to the northward, he describes other islands, which have been sometimes understood as the Azores. But he specially states that one of these islands is near the shore of Morocco; and it seems more likely that his account skips about capriciously from one part of the eastern archipelago to another. It may be said that his hero Dzou l' Carmain is a traditional blending of Hercules and Alexander.

He tells us of the Island Sara situated near to the Sea of Darkness, where Dzou l' Carmain landed before the shadows had covered the face of the sea and remained one night; and the inhabitants attacked him and his companions with blows from stones and

wounded many. Their canine teeth project from their mouths, their eyes scintillate like stars and their cheeks look like burnt wood.

Another island of the same sea is called the island of the feminine devils, the men having no beard and the two sexes differing little in appearance. Their clothes are the leaves of trees. They speak an unintelligible language and make war on marine monsters. This may imply the use of boats. If so, it stands alone as such a nautical allusion. But perhaps the inference is unnecessary. "Monsters" would no doubt include whales, and Fayal of the Azores has continued until now a center of the whaling fishery.

Next, the Island of Delusion is of considerable size, dominated by a mountain, on the side of which live short brown men wearing a beard that descends to their knees and feeding on spontaneous vegetation such as nourish the animals. A river of sweet, fresh water descends from the mountain. The requirements of this island are met by Teneriffe, except the beards and the apparent limitation to a single stream. They may be met still more completely by Pico of the Azores; but we do not know of inhabitants there from any other source.

The Island of El Ghauer, equally large, abounds in all kinds of herbs and plants. Mentions strong places of retreat for the savages and cattle, the latter having horns of great length. Grand Canary and its caverns would suit this very well, if for the cattle we read goats—not otherwise.

The Island of Al Mustackhin, isle of the suppliants, has a citadel dominating the city, for the isle is populous, having mountains, rivers, trees, fruits and cultivated fields. Edrisi relates the victory of Dzou l' Carmain over a dragon which had been devastating this island. There seems no clue to the latter and the citadel is probably as mythical as the dragon.

Another island is named Calhan, the inhabitants of which have human forms and animal heads. Perhaps baboons or gorillas encountered on some island off the African coast may be responsible for the grotesque fantasy. In that case the island would probably belong to a more southern region than the Canaries and would be closer in shore.

Another isle is that of the two brother magicians. They were pirates. To punish them God turned them to rocks on the sea shore. They are situated opposite the port of Asafi and at a distance which permits the smoke from the isle to be seen from the continent when the air is perfectly clear.

This is related in particular by Abmed Ibn Omar, an Arabic naval commander who wished to take his fleet there and land, but death surprised him before he could do so. Information as to the naming of Asafi and other curious items is derived from the report of the Maugrurin, who sailed from Lisbon. Saffi or Asifi is about opposite the Canary Islands. Lanzarote would be nearest. The smoke of the volcano of Teneriffe may be meant.

In this sea is an island of vast extent surrounded by deep shadows. They call it the Isle of Sheep because there are enormous flocks of them. These animals are small and their flesh is so bitter that it cannot be eaten.

Edrisi again refers to the Maugrurin narrative in corroboration.

This Isle of Sheep cuts a great figure in Mediaeval romancing. In the earliest map, 1325, showing the Island of Brazil west of southern Ireland, an alternative name "Montonis" there inscribed means "rams" in Italian; unless it should rather be read Montanis for Mountains. But whether the Isle of Sheep or the Isle of Rams represents any real land is simply a puzzle unsolved. The same may be said for the strange savor of their flesh.

Near this isle is the isle of Raca or Isle of the Birds, a species like eagles which feed on marine animals.

He quotes the *Book of Wonders* for an account of an expedition sent by

a king of France to this island for the birds and for a fruit which is an antidote for poison; but the fleet was wrecked and never returned.

The Azores as an archipelago are said to have their name from the great number of hawks or buzzards found there. In one of the fifteenth century maps the Isle of Falcons appears as one of the newly rediscovered members of that widely dispersed group. Corvo, the crow, has substantially held its name since its first appearance on the maps in the middle of the fourteenth century—probably meaning cormorants. Putting these things together it

seems likely that an Isle of Birds should be looked for especially in this part of the sea. Also the Pizagano map of 1367 represents a Breton fleet undergoing disaster from shipwreck, dragons, and octopi at an island southwest of the circular Island of Brazil, which again is southwest of Ireland, the first mentioned island being also far northwest of Terceira of the Azores. It is at least possible that this may be the expedition from France referred to by Edrisi.

Edrisi's greatest Atlantic island seems to be Saisland—by one conjecture Iceland, but more probably a magnified Madeira. He says it is fifteen days in length, two in breadth. There have been civil wars there, but it is still populous with cities. This last statement has never been true of any island in the Atlantic, as far as we know. The most reasonable conclusion is that Saisland, if real, has been exaggerated and glorified in Edrisi's account quite out of recognition. The earlier description of Diodorous may have misled him.

Laca is another island of this sea. It produces a great quantity of aloes. Merchants repair thither for it and sell it to the kings of Western Morocco.

This carries us into southern waters again, but we really know nothing about Laca.

It seems idle to attempt to identify each one of Edrisi's islands and perhaps his own mind was not very clear concerning them. Arab traders from Morocco, Arab naval expeditions, French records or rumors, explorers of the Maugrurin kind and myth-makers of a much earlier time all had contributed something to his vision of the Atlantic islands, which he would like to pass on to us. But he really seems to have gathered some valuable facts. The Portuguese, when discovering or rediscovering Madeira and the Azores in the fifteenth century, did not report any inhabitants; but Edrisi did, and the natural probabilities of the situation would seem to call for them. A really thorough archaeological and anthropological search of all this group is a great desideratum, and this is especially true of Corvo, nearly halfway across the Atlantic toward America.

Edrisi has been most widely known for his narrative of the Maugrurin, sea wanderers who left the port of Lisbon, necessarily

after the Moors captured it but evidently long before the time of his writing, with the resolve to penetrate to the end of the Sea of Darkness.

They reached the Isle of Sheep, of which so much is made in so many narratives of different nations, and which may be one of the larger Azores islands. Afterward they became entangled in the weedy sea and were glad to turn southward. In the end they were detained by the chief of a populous island, who had an Arabic interpreter and reported a voyage of his father for some forty days beyond his island, without finding an end of the sea. He had them conveyed to the African coast, which was not very distant, and they found their way back to Lisbon, where a street was named after them. If any faith may be put in the tale, they had demonstrated the extension of Arab influence and language in some slight degree to the nearer members of the Canary group; but they seem to have added no other item of anthropologic value.

The Maugrurin legend itself in general outlines may very likely be true, for it was quite certain that some of the maritime Arabs would push out from the Iberian coast in a venturesome manner, drawn by the mystery of the shadowy sea and perhaps also by rumors of Christian fugitives who had sought refuge on remote islands with great wealth of every kind—legends of the Island of the Seven Cities, which endured for many centuries afterward, influencing exploration and geography, and of which no one can positively say that there is no germ of truth in them.

We do not know whence Edrisi derived this Moorish tale. It is one of his sources; but far from the only one. Obviously these were heterogeneous in nature and of quite different value. He believes in advanced civilization in some islands and of beast-headed people in others; but his varying accounts of savagery or pastoral habits, beards or smooth faces, brown skins and small stature, and of fig leaves for garments all have a ring of reality. Perhaps the first meetings of white men with insular Berbers and modifying elements are recorded in these notes of Edrisi.

The Irish Inrama or sea-sagas had a very early origin, but grew and changed during their long pre-Columbian life. They

certainly indicate a knowledge of Atlantic islands and a sense of their loveliness; but the human island population to which they introduce us is conventional and unilluminative. We hear of monasteries and hermits, of enchantresses and marooned navigators, of Enoch and Elijah on one island and the tortured Judas on another; but after much traveling among monsters and adventures we find surprisingly little to indicate observation of the real inhabitants of any real islands remote from Hibernia. The voyage of Snedgus gives us again the beast-headed people of Calhan; huge birds figure in the Voyage of St. Brandon; elsewhere in the latter some small folk are mentioned and these have been conjectured to be Eskimo; but there seems no warrant for the guess and no probability that the voyage was supposed to be in a northern direction. Bran, Maelduin, the Sons of Corra, Snedgus and St. Brandon all ranged the sea, according to romancers, and visited a multitude of islands, but they tell us very little of the real people which seems authentic, except that they kept sheep and must have lived under more or less volcanic conditions in many places.

If there are only very faint indications of pre-Portuguese populations in the Azores and Madeira, we are rewarded with quite a wealth of information as to the Canary islands from the fourteenth and fifteenth centuries.

In the year 1341, certain Italian letters, discovered long afterward among official records, were written in Florence, Italy, by certain Florentine merchants dwelling in Seville, Spain. The translation is by R. H. Major in his introduction to *The Canarien*. They relate that,

> On the first of July of that same year two vessels furnished by the King of Portugal, accompanied by a smaller vessel well armed and manned by Florentines, Genoese, Castilians and other Spaniards, set sail for Lisbon and put out into the open sea. They took with them horses, arms, and warlike engines for storming towns and castles, in search of those islands commonly called the "Rediscovered." . . . They did not return until November, when they brought back with them four of the natives, a large quantity of goat skins, the fat and oil of fish, and seal skins; red wood which dyed almost as well as the vergino (Brazil wood) although connoisseurs pronounced it not to be the same; the barks of trees to stain with a red color; red earth and such other like things.

The pilot, a Genoese, estimated the distance nine hundred miles from Seville; but the distance from Cape St. Vincent was much less. Their first island, probably Fuerteventura, was one mass of uncultivated, stony ground, but full of goats and other beasts and inhabited by naked men and women, who were like savages in their appearance and demeanor. He added that he and his companions obtained in this island the greater part of their cargo of skins and fat, but they did not dare to penetrate far into the country.

Passing thence into another island (probably Grand Canary) somewhat larger than the first, a great number of natives of both sexes, all nearly naked, came down to the shore to meet them. Some of them who seemed superior to the rest were covered with goat skins, yellow and red, and as far as could be seen from a distance the skins were fine and soft and tolerably well sewn together with the intestines of animals. To judge from their gestures, they seemed to have a prince, to whom they showed much respect and obedience. Their language was soft and their pronunciation rapid and animated like Italian. On the northern coasts of the island, which were much better cultivated than the southern, there were a great number of little houses, fig trees, and other trees, palm trees which bore no fruit, and gardens with cabbages and other vegetables. Here twenty-five of the sailors landed and found nearly thirty men quite naked, who took to flight when they saw their arms. The buildings were made with much skill of square stones covered with large and handsome pieces of wood. Finding several of them closed, the sailors broke open the doors with stones, which enraged the fugitives, who filled the air with their cries. The houses were found to contain nothing beyond some excellent dried figs, preserved in palm baskets like those made at Cesena, corn of a much finer quality than the Italian, not only in the length and thickness of its grain, but its extreme whiteness, some barley and other grains. The houses were all very handsome and as clean inside as if they had been whitewashed. The sailors also came upon a chapel or temple in which there were no pictures or ornament, but only a stone statue representing a man with a ball in his hand. This idol, otherwise naked, wore an apron of palm leaves. They took it away and carried it to Lisbon. On leaving this island they saw several others. . . . They afterward saw other islands making in all thirteen, some of them inhabited and some not. . . . The languages of these people were said to be so different that those of one island did not understand another, and they had no means of communication except by swimming. . . . The four men whom they carried away were young and beardless and had handsome faces. They wore nothing but a sort of apron made of cord from which they hung a number of palm or reed fibers of a hair's breadth and a half or two hairs' breadths which made an effectual covering. They were uncircumcised. Their long light

hair veiled their bodies down to the waist and they went barefooted. . . . They did not exceed their captors in stature, but they were robust of limb, courageous and very intelligent. When spoken to by signs they replied in the same manner. . . . They sang very sweetly and danced almost as well as Frenchmen. They were gay and merry and much more civilized than many Spaniards. . . . They absolutely refused wine and only drank water. Wheat and barley they ate in plenty as well as cheese and meat which was abundant in the islands and of good quality; for, although there were no oxen, camels, or asses, here were plenty of goats, sheep, and wild hogs. They were shown some gold and silver money, but they were quite ignorant of the use of it, and they knew as little of any kind of spice. Rings of gold and vases of carved work, swords and sabres were shown to them, but they seemed never to have seen such things and did not know how to use them. They showed remarkable faithfulness and honesty, for if one of them received anything good to eat, before tasting it, he divided it into portions which he shared with the rest. Marriage was observed among them and the married women wore aprons like the men, but the maidens went quite naked, without consciousness of shame.

In 1382 an expedition under Captain Lopez was driven by storm to Grand Canary and he and twelve companions dwelt there seven years, taking care of flocks granted them and teaching the islanders Christianity. The latter, however, turned upon and killed them at the last, it is said on account of alleged hostile communications "to the land of the Christians." One of the victims first, however, wrote a warning letter which was given by a convert to the first European expedition which followed. In 1402 Bethencourt began the conquest of the Canaries, which was not completed until long after his death, by other hands and in the latter part of that century.

Gomez Eames de Azurara in 1448 compiled a narrative by one of Prince Henry's sailors of the Conquest of Guinea, reciting how a Portuguese expedition of 1443 took part in a slave raid on the Canarian island of Palma, wherein the Palma fighting men "hurled stones and lances with sharp horn points at them with great strength and precision." One of the women captured "was of extraordinary size for a woman and they said that she was the queen of a part of the island."

Azurara explains that "The Grand Canary" was ruled by two kings and a duke, but the real governors were an assembly of knights filled up by election from the sons of their own class.

Their only weapons were a short club and stones. . . . They had wheat but had not the skill to make bread, but ate the meal with meat and butter. . . . They held it an abomination to kill animals and employed Christian captives as butchers whenever they could get them. They kindled a fire by rubbing one stick against another. They believed in a God who would reward and punish, and some of them called themselves Christians.

The people of Gomera were less civilized. Their women were regarded almost as common property. . . . They made their sister's sons their heirs. They lived chiefly on milk, herbs, and roots. They ate also filthy things like rats and vermin.

The people of Teneriffe were much better off and more civilized. They had plenty of wheat and vegetables, pigs, sheep, and goats, and dressed in skins. They had, however, no houses but passed their lives in huts and caves. Their chief occupation was war and they fought with lances of pine wood made like great darts very sharp with points hardened in the fire. There were eight or nine tribes, each of which had two kings, one dead and one living, for they had the strange custom of keeping the dead king unburied till his successor died and took his place. The body of the former was thrown into a pit. They were strong and active men and had their own wives and lived more like men than some of the other islanders. They believed in a God.

The people of Palma had neither bread nor vegetables, but lived on mutton, milk and herbs. They did not even take the trouble to catch fish like the other islanders. They fought with spears like the men of Teneriffe, but pointed them with sharp horn instead of iron, and at the other end they also put another piece of horn but not so sharp as that at the point. They had some chiefs who were called kings. They had no knowledge of God nor any faith whatever.

Alvisare Cadamosto, a Venetian in the service of Prince Henry the Navigator of Portugal, as cited by Major in said introduction, reported in 1455 concerning these islands:

Four of them, Lanzarote, Fuerteventura, Gomera and Ferro were inhabited by Christians, the other three, Grand Canary, Teneriffe, and Palma by pagans. . . . They had barley bread, goats' flesh, and milk in plenty for goats were very numerous; they had no wine nor corn except what was imported, and the islands produced but little fruit. There were great numbers of wild asses especially in the island of Ferro. Great quantities of orchil for dying were sent from these islands to Cadiz and Seville and thence to other parts both east and west. Their chief products were goats' leather very good and strong, tallow and excellent cheeses. The inhabitants of the four Christian islands spoke different languages so that they could with difficulty understand each other. There were no forti-

fied places in them, only villages, but the inhabitants had retreats in the mountains to which the passes were so difficult that they could not be taken except by a siege. Of the three islands inhabited by pagans (in 1455) two were the largest and most populous of the group: namely, the Grand Canary, in which there were about eight or nine thousand inhabitants, and Teneriffe, the largest of all, which contained from fourteen to fifteen thousand. Palma was not so well peopled, being smaller, but a very beautiful island. The Christians have never been able to subdue these islands, as there were plenty of men of arms to defend them and the mountain heights were difficult of access. Teneriffe was governed by nine chiefs, who did not obtain possession by inheritance but by force. Their weapons were stones and javelins pointed with sharpened horn instead of iron and sometimes the wood itself hardened by fire till it was as hard as iron itself. The inhabitants went naked except some few who wore goat skins. They annointed their bodies with goat's fat mixed with the juice of certain herbs to harden their skins and defend them from cold, although the climate is mild. They also painted their bodies with the juice of herbs green, red, and yellow, producing beautiful devices, and in this manner showed their individual character much as civilized people do by their style of dress. They were wonderfully strong and active, could take enormous leaps and throw with great strength and skill. They dwelt in caverns in the mountains. They had some fruits, chiefly figs, and the climate was so warm that they gathered in their harvest in March or April. They had no fixed religion but some worshipped the sun, some the moon and others the planets with various forms of idolatry. Each man might have as many wives as he liked. These accounts were from the Christians of the four islands, who would occasionally go to Teneriffe by night and carry off men and women, whom they sent to Spain to be sold as slaves. Another of their customs was that when one of their chiefs came into possession of his estate some one among them would offer himself to die in honor of the festival. On the day appointed they assembled in a deep valley, when after certain ceremonies had been performed the self-devoted victim threw himself from a great height into the valley and was dashed to pieces. The chief was held bound in gratitude to do the victim great honor and to reward his family with ample gifts.

Bethencourt's expedition of 1402 was accompanied by the Chaplains Bontier and Leverrier, who prepared a joint narrative known as *The Canarien*. Their main interest is in the quarrels and adventures of the conquerors, but they supply many items about the conquered or resisting people. We are told of the great physical prowess of the King of Lanzarota, who six times freed himself from his captors, of the caverns in which the natives of that island took refuge, of the beauty and modesty of the Lanzarote women, who wear long leather robes reaching down to the ground and

most of whom have three husbands each, of the raids before this time by Spaniards and Corsicans on the people of this island. We hear also of the large stature and powerful build of the men of Fuerteventura, "who are difficult to take alive," of their notable fortifications, one being a strong wall across the island, of their attachment to their own forms of government, and the superlative quality of their cheeses. Also of the "very fine race of Ferro," both men and women, and their great abundance of dogs, sheep and goats; the delightfulness of Palma, its fine people and their exclusive flesh diet; the tallness of the Gomera islanders, their most difficult language spoken mainly with the lips as if they had no tongue; of the Teneriffe Guanche, the hardiest race in all these islands; of the Grand Canary husbandmen who cultivate all kinds of grain and are enthusiastic fishers as well, whose men are handsome and well formed and whose women are beautiful. Those various accounts are mutually corroborative in their main lines, though materially differing more or less in detail even when treating of the same island, the discrepancies being in part accounted for by the lapse of time between observations and the intervening raids, importations and changed conditions. Like Edrisi's much earlier notes, they agree as to great local dissimilarities in matters of comfort and civilization; but we find no evidence that any of these island people had risen as high as the denser American populations—Inca, Aztec or Maya—or much higher than the more promising Indian tribes within the borders of the United States. They had been long separated, island from island, forgetting the art of navigation which had once brought them there, probably in at least two distinct waves, with a wide interval between them; so that the local dialects or languages were hardly mutually intelligible. This loss of navigation of course implies a considerable declension in general culture, but it must be said that we hear of no evidences of great cities and other enduring monuments of civilization such as the ancient writers quite freely depicted.

The local differences may well have been due in part to a lack of homogeneity of race. We know of many raids by white men and of some temporary colonizations lasting for years. Similar tales

are told of the Arabs. Major finds in the facts of divergence evidence of a subsequent Arab wave of population impinging on the Berber aborigines and obtaining control of the islands nearest the African coast, but affecting more slightly those which are remoter and greater. But this does not seem to meet all the requirements of the case and Father Espinosa who wrote while there were still living Guanche on Teneriffe, where he long made his home, contrasts the rather brown and usually nearly naked aspect of the southern shore natives with the white and rosy attractiveness of those on the northern side, whose women were beautiful. One would not think of Arabs in just that way and must be inclined to attach some weight to traditions of Gothic or other Aryan immigrants, without denying the Arabic admixture in other quarters, perhaps on a larger scale. Nevertheless it is likely that the Berber substratum remained the mass of the population.

Perhaps there is nothing about these people more distinguishing or significant than the use habitually made of caverns in some of the chief islands, and the preservation of the dead as mummies especially on the most populous island, Teneriffe. In Lanzarota the caverns were mainly temporary places of refuge in time of sudden danger. In Teneriffe some of them were more regular and permanent abodes, others served as mortuaries for the deposit of dead generations after the manner of the catacombs. The caverns seem to link them, perhaps by coincidence only, with the troglodytes of northern Africa and certain Indian tribes of our cordilleras in the southwest. The habit of preserving corpses as mummies in similar fashion links them on the one hand to the Nile and on the other to the land of the Inca. But again it may be a mere matter of chance or the result of like requirements, conditions, and impulses in spots.

Taking a general survey of the field, we have no evidence of human occupancy in Iceland other than Celtic and Norwegian, though there are faint indications pointing that way; that the case for the Azores and Madeira is much the same, apart from occasional European visitors from the mainland, though certain matters of nomenclature and the probabilities of the situation

suggest an early native population; but that there is ample and detailed information concerning a native population of the Canaries who must have reached their island homes by navigation in remote times, who occupied stations well advanced toward America on the route first followed by Columbus and who present many traits, customs and characteristics which remind one of the North American Indians. But of course these may have grown out of similar conditions of primitive tribal life and may not imply any community of descent.

WASHINGTON, D. C.

ANTHROPOLOGY AT THE PHILADELPHIA MEETING WITH PROCEEDINGS OF THE AMERICAN ANTHROPOLOGICAL ASSOCIATION FOR 1917

By WILLIAM CURTIS FARABEE

The nineteenth meeting (sixteenth annual) of the American Anthropological Association was held December 27–28, 1917, at the University Museum, Philadelphia, in affiliation with the American Folk-Lore Society.

The council meetings were attended by Kroeber, Gordon, Swanton, MacCurdy, Goddard, Moorehead, Peabody, Michelson, Nelson, Parsons, Hrdlička, Wissler, Speck, Goldenweiser, Wardle and Farabee. President A. L. Kroeber occupied the chair.

Report of the Secretary

The Proceedings of the last annual meeting of the American Anthropological Association were published in the *American Anthropologist* for January–March 1917. There has been no special meeting of the Association nor of the Council during the year.

One member has died, Dr. Juan B. Ambrosetti of Buenos Aires. Many of the members will remember with pleasure Dr. Ambrosetti's visits to this country in 1902 and 1912. South America has lost in his death one of its leading anthropologists and all Americanists a distinguished fellow-worker.

The Secretary, Alfred M. Tozzer was called into the service of the Government as Captain in the Aviation Section, July 1917, and William Curtis Farabee was appointed by the Executive Committee to fill out the unexpired term.

The fifty-six applicants for membership submitted by the Secretary were all elected, making a total membership of 455. The revised list of members with addresses is published elsewhere in this issue.

Report of the Treasurer

Receipts and Disbursements, January 1, 1917, to December 31, 1917.

Receipts

Balance on hand, January 1, 1917			$195.75
Anthropological Society of Washington:			
Balance unpaid, January 1, 1917	$ 47.25		
American Anthropologist, vol. XVIII, no. 4	47.25		
American Anthropologist, vol. XIX, nos. 1–3	127.81	$ 222.31	
Less amount still unpaid		87.56	134.75
American Ethnological Society:			
American Anthropologist, vol. XVIII, no. 4	63.88		
American Anthropologist, vol. XIX, nos. 1–3	212.64		276.52
Annual membership dues:			
Account of 1915		6.00	
" " 1916		55.00	
" " 1917		1,873.00	
" " 1918		154.00	2 088.00
Subscription to *Anthropologist*, unpaid Jan. 1, 1917.			4.05
Sale of publications:			
Amount unpaid January 1, 1917	34.47		
Less amount still unpaid	30.85	3.62	
Current sales	193.14		
Less amount still unpaid	41.78	151.36	154.98
From Yale University, for *Memoirs*, vol. III, no. 2			518.60
Gift to the Association from Prof. H. Montgomery			4.00
Miscellaneous, including overcharges			16.79
Total receipts			$3,393.44

Disbursements

Printing and distribution of publications:			
Amount unpaid January 1, 1917	$1,392.60		
American Anthropologist, vol. XVIII, no. 4	521.72		
American Anthropologist, vol. XIX, nos. 1–3	1,111.83		
Memoirs, vol. III, nos. 2–4	1,227.49		
Memoirs, vol. IV, nos. 1–2	427.71		
	4,681.35		
By credit, sale of old metal, etc.	64.27	$4,617.08	
Less amount still unpaid		2,117.08	2,500.00

Illustrations for publications:			
American Anthropologist, vol. XVIII, no. 4	.90		
American Anthropologist, vol. XIX, nos. 1–4	155.87	156.77	
Memoirs, vol. III, no. 4		10.98	167.75
Expenses of the Editor			348.95
Expenses of the Treasurer			73.48
Expenses of the Secretary			33.70
Miscellaneous, including refund of overcharges			9.85
Total disbursements			3,133.73
Balance in Washington Loan & Trust Co.			259.71
			$3,393.44

PERMANENT ACCOUNT

Receipts

Previously acknowledged	$1,200.00

Disbursements

Loan to General Fund	$1,100.00
Balance in Munsey Trust Company	100.00
	$1,200.00

NEIL M. JUDD, *Treasurer.*

The accounts of the Treasurer, Neil M. Judd, have been examined and found correct.

Signed:
TRUMAN MICHELSON
A. HRDLIČKA.
Auditing Committee.

WASHINGTON, D. C.,
January 2, 1918.

REPORT OF THE EDITOR

There have been issued this year four numbers of the *American Anthropologist;* namely, the fourth number of volume 18 and the first three numbers of volume 19. No. 4 of the current volume is entirely in page proof with the exception of the Index which is now being made up into pages. Volume 19 contains, with the exception of the Index, 591 pages and XV plates. The *Memoirs* were quite behind a year ago, since which time there have been issued two *Memoirs* of volume III and two numbers of volume IV. The third and fourth numbers of the current volume are now printing and

will be issued within a few days. The size of the numbers of the *Memoirs* is rather difficult to control because the longer papers are segregated for issue in the series and they vary considerably in length. Dr. Hatt's *Memoir* which was no. 3 of vol. III was 101 pages. No. 4 of this volume was a rather long article by Dr. Thurnwald and contains 141 pages. Volume IV will contain 327 pages, exclusive of the Index. The fact that the entire volume is without illustrations has very largely influenced the cost of the volume.

Valuable papers are constantly being offered both for the *Memoirs* and the *American Anthropologist*. It has been necessary to decline certain of these because of the excessive cost involved in their publication. For example, a paper apparently of great value dealing with the statistical measurements of women has been postponed from time to time, hoping means for publication might be available from outside sources. The present size of the *Anthropologist* is just about sufficient to care for the more important articles for which the Association should feel itself particularly responsible because either of the character of the articles, the circumstances under which they are presented, or the relationship of the authors to the Association. The Editor has tried to divide the available space justly between the various subjects and interests represented by the members.

Considering the present condition of the finances of the Association, the high cost of the production of the publications, and the uncertainty of the era under which we are existing the Editor feels compelled to recommend certain economies. Considering our obligations to a large number of institutions who have become members of the Association, it does seem that we can discontinue the publication of the *Memoirs* or reduce the number of issues. With justice however the number of pages can be considerably reduced. As long as present circumstances continue it is recommended that the *Anthropologist* be kept to 500 pages per volume, and the *Memoirs* to as small a compass as is possible and still issue four numbers, that is from 150 to 250 pages per volume.

PLINY E. GODDARD, *Editor*.

The Council took affirmative action on the following:

1. That the committee consisting of Boas (Chairman), MacCurdy, Goddard, Dixon, Tozzer, Hooton, Kroeber, and Speck, on "Teaching of Anthropology in the United States" be made permanent and that they make annual reports to the Council. That MacCurdy publish his report on "The Number of Institutions in the United States giving Instruction in Anthropology" at the earliest opportunity.

2. That the question of the finances of the Association be referred to the Executive Committee with power to act.

3. That "The American Anthropological Association notes with satisfaction the official recognition of Anthropology by the Government of the United States by the appointment of a Committee to represent that subject on the Council of National Defense. It is the belief of the Association that the services of Anthropology may be useful to the Government at this time and notes with satisfaction that the Committee has already made practical suggestions for the guidance of the Government along several lines. The Association stands ready to cooperate with the Committee of the National Research Council on behalf of the Government."

4. That the report of the Committee on Nominations, consisting of Swanton, Speck, and Goddard, be accepted. The election of officers resulted as follows:

President: A. L. Kroeber, Affiliated Colleges, San Francisco.

Vice-President, 1918: B. Laufer, Field Museum of Natural History.

Vice-President, 1919: John R. Swanton, Bureau of American Ethnology.

Vice-President, 1920: George Grant MacCurdy, Yale University.

Vice-President, 1921: Aleš Hrdlička, U. S. National Museum.

Secretary: Alfred M. Tozzer, Harvard University.

Acting Secretary: William C. Farabee, University of Pennsylvania.

Treasurer: Neil M. Judd, U. S. National Museum.

Editor: Pliny E. Goddard, American Museum of Natural History.

Associate Editors: John R. Swanton, Robert H. Lowie.

Executive Committee: The President, Secretary, Treasurer, and Editor (*ex officio*), Clark Wissler, Edward Sapir, G. B. Gordon.

Council: F. Boas, W. H. Holmes, F. W. Fewkes, R. B. Dixon, F. W. Hodge, A. L. Kroeber, George B. Gordon, B. Laufer, John R. Swanton, G. G. MacCurdy, A. M. Tozzer, Neil M. Judd, P. E. Goddard (*ex officio*); Alice C. Fletcher, C. P. Bowditch, S. Culin, R. H. Lowie, C. H. Hawes, E. Sapir, N. C. Nelson, H. Bingham, J. A. Mason, H. K. Haeberlin, E. W. Gifford, Elsie Clews Parsons, J. P. Harrington (1918); A. E. Jenks, S. A. Barrett, W. Hough, A. Hrdlička, B. T. B. Hyde, C. Wissler, F. G. Speck, A. A. Goldenweiser, E. A. Hooton, A. V. Kidder, F. C. Cole, L. Spier, L. J. Frachtenberg (1919); Byron Cummings, W. C. Farabee, G. G. Heye, H. J. Spinden, S. M. Barbeau, W. D. Wallis, A. B. Lewis, S. Hager, Miss H. N. Wardle, Theodoor de Booy, F. H. Sterns, S. K. Lothrop, R. T. Aitken (1920); W. C. Mills, H. Montgomery, C. B. Moore, W. K. Moorehead, E. K. Putnam, C. Peabody, C. C. Willoughby, T. Michelson, A. B. Skinner, M. H. Saville, E. W. Hawkes, Louis Sullivan (1921).

The President, Professor A. L. Kroeber, appointed the following committees:

Committee on Program: R. B. Dixon (chairman), Elsie Clews Parsons, Aleš Hrdlička, A. A. Goldenweiser, Berthold Laufer, P. E. Goddard (*ex officio*).

Committee on Finance: Charles Peabody (chairman), Edward E. Ayer, Charles P. Bowditch, William H. Furness, George G. Heye, Clarence B. Moore, Homer E. Sargent.

Committee on Policy: Franz Boas (chairman), Roland B. Dixon, J. Walter Fewkes, W. H. Holmes, Robert H. Lowie, George G. MacCurdy, A. M. Tozzer.

Committee on Publication: A. L. Kroeber (chairman *ex officio*), Hiram Bingham, Stewart Culin, P. E. Goddard (secretary *ex officio*), A. A. Goldenweiser, G. B. Gordon, Walter Hough, Neil M. Judd, F. W. Hodge, Berthold Laufer, E. Sapir, M. H. Saville, John R. Swanton, A. M. Tozzer, W. H. Holmes, R. B. Dixon, F. Boas.

To represent the Association on the Council of the A. A. A. S.: W. H. Holmes and Franz Boas.

5. That Dr. A. Hrdlička's name be submitted to the American Association for the Advancement of Science for election as Vice-President of the Section H.

6. That the Association hold its next regular meeting at Boston.

7. That "The American Anthropological Association and the American Folk-Lore Society express their appreciation of the hospitality extended by the University of Pennsylvania in tendering the use of buildings for the holding of their scientific meetings and for the luncheon and supper-smoker on Friday, December 28. The thanks of the Societies are also given to Dr. and Mrs. W. P. Wilson for the charming Cliff Dwellers' Luncheon at the Commercial Museum on Thursday, December 27.

Addresses and Papers

Dr. George Byron Gordon, Director of the University Museum, delivered the opening address. Professor A. L. Kroeber's Presidential address before the Association was entitled "Centers and Areas of Culture on the Pacific Coast."

The list of authors and titles of papers presented follows:

Colton, H. S. Some Small House Ruins in the Coconino Forests, Arizona.

Day, Lewis Clinton and Stamp, Harley. The Treatment of Makis Nez Batcilla Bega of Se Le Daskon.

Farabee, W. C. The Marriage of the Electric Eel.

Frachtenberg, L. J. (a) What are the true Criteria for establishing Linguistic Relationships? (b) Abnormal Types of Speech in Quileute. (c) The Eschatology of the Quileute Indians.

Goddard, P. E. The Criteria of Genetic Linguistic Relationship.

Goldenweiser, A. A. Form and Content in Totemism. The Mind of the Eskimo.

Hrdlička, A. Report of the Committee on Anthropology of the National Research Council. The Growing Need of a Separate American Journal of Physical Anthropology.

Kroeber, A. L. Pre-American Culture of the Great Basin as Represented by Collections from a Nevada Cave.

MacCurdy, G. G Surgery among the Ancient Peruvians.

Merwin, B. W. Notes on Chiriqui Pottery Design.

MICHELSON, TRUMAN. Potawatomi Notes. Hartland's Theory of the Priority of Matrilineal Kinship.

PARSONS, E. C. Christian Rites in Zuñi Ceremonialism.

SPECK, FRANK G. The Functions of Wampum among the Northeastern Algonkins. Bird Lore of the Northeastern Algonkins.

STAMP, HARLEY. Northwestern and Siberian Folk Tales.

SWANTON, J. R. Resemblances between the Chitimacha and Tunica Languages.

WARDLE, H. NEWELL. Iron-ore Artifacts from Alabama. Note on the Ground Hog Day Myth and its Origin.

WILDER, H. H. Restorations.

WILSON, L. L. W. Report on Excavations at Otowi, New Mexico, Summer of 1917.

BOOK REVIEWS

METHODS AND PRINCIPLES

Culture and Ethnology. ROBERT H. LOWIE. Douglas C. McMurtrie: New York, 1917. 190 pp.

Although the methods and principles of modern ethnology, as conceived and cultivated in America, are well known to the small band of workers, a coherent and concise summary of the ideas for which the science of ethnology stands, and which could serve for the information of a wider public, has hitherto been lacking. This long-felt want is felicitously supplied by Dr. Lowie's booklet which has grown out of a series of lectures delivered in 1917 in the American Museum of Natural History. The cause of ethnology is here pleaded with eloquence and well-balanced judgment, and a difficult subject is presented in an attractive and lucid exposition. This work will form an excellent introduction and guidance to the raw recruit along the thorny path of ethnological research, and it will be read by the veterans with as much pleasure and profit. It should be the permanent possession of every one, together with Boas's *Mind of Primitive Man* and Wissler's new book on *The American Indian*. So many confusions, misconceptions, and prejudices in regard to what ethnology constitutes are still prevelant, even in the minds of scientists in general, especially among historians, orientalists, art-students, etc., that a book like this one will be warmly welcomed and will help to clarify the atmosphere. Dr. Lowie's universal reading, his keen analytic sense that goes straight at the root of things, and his gift of clear representation, insure his pages a piquant flavor and the constantly growing sympathy of the reader.

In his first lecture, the author endeavors to determine the relation of culture to psychology and to defend the rights of ethnology as an independent science. Psychology, as he aptly expresses it, is as impotent to reduce to really interpretative psychological principles the subjective aspect of cultural phenomena as it is to explain the historical sequence of events, and the principles of psychology are as incapable of accounting for the phenomena of culture as is gravitation to account for architectural styles. Over and above the interpretations given by psychology, there is an irreducible residuum of huge magnitude that calls for special

treatment and by its very existence vindicates the *raison d'être* of ethnology; but we need not eschew any help given by scientific psychology for the comprehension of specifically psychological components of cultural phenomena. At this point I wish the author might have made some reference to the growing importance of sexual psychology without the aid of which numerous factors in the life of all peoples cannot adequately be grasped. No sinologue, for instance, has ever been able to advance a correct explanation of the reasons why the Chinese bind (or used to bind) the feet of their women: this is a sex problem to which no historian or ethnologist (not to speak of the Chinese themselves) can make an answer. Moreover, it seems to me that for practical working purposes we are compelled to postulate the existence of a social or national psyche, whether this be correct in a strictly scientific sense or not, this psyche to be conceived, of course, as being composed of ideas and habits historically acquired and developed, and accordingly as a changeable quantity. It is a commonplace observation that the Japanese adopted and absorbed the fundamental principles of Chinese civilization; it is still more interesting to note what they did not adopt from their superior neighbors: they did not take over their costume, their domestic architecture, their furniture and kitchen, sitting on chairs and at tables (in fact retained all their ancient material possessions relative to domestic life), foot-binding, consumption of opium, not to mention numerous other customs and practises. An answer to this why has not yet been given by any ethnologist or historian of Japan. Why did Christianity conquer the peoples of the Indo-European stock, while it did not appeal to the Semites in the midst of whom it was born? Why could it never obtain any permanent influence on any nation of Asia? Why is Buddhism, the product of an Aryan society, extinct in India, the land of its birth, while it flourishes among so many non-Aryan peoples of central and eastern Asia? Or why do the tribes of Africa so easily become Mohammedanized? For each of these questions, hundreds of historical facts can be marshalled to state the merits of the particular case; but these do not bring us nearer to a satisfactory explanation. Here, and in similar problems, it seems to me the application of psychological methods or viewpoints will remain unavoidable. On the other hand, each cultural fact reacts on the tribal or national psyche, and these effects have as yet been little studied. To illustrate, among the European and many Asiatic tribes family life has always centered around the fire of the kitchen or tent; the kitchen is still the bond that unites the members of the family in our civilization (as far as they are not cor-

rupted by the institution of the family hotel). The formation of the family in any other way seems almost inconceivable to us. Yet the Chinese in this respect are radically different from others. The kitchen never formed the center of their house; on the contrary, it was always far removed from it as a separate building and entrusted to the care of male servants. There are no female cooks, and the mother of the house avoids the kitchen, where the family never assembles. While the organization of the Chinese family outwardly is very much like our own, yet the forms of family life and the psychical relations of the family members are diverse, and this difference seems to be directly traceable to influences of material causes, as given by the arrangement of habitations and the location of the kitchen.

The second lecture, "Culture and Race," forms especially attractive reading, and there is no doubt that every ethnologist will subscribe to the author's clear exposition and his statement of the relation of culture to racial traits: culture cannot be adequately explained by race, and the same race varies extraordinarily in culture even within a very narrow space of time. The Chinese and some of our American Indians, such as the ancient Central Americans and Peruvians, did attain a very high level, which may be equated with that of Europe at a relatively recent period. No term has been more misused and sinned against than the catchword "racial mentality." The anthropologist is inclined to assume that mental powers are fairly uniform through mankind, but work in different directions or are projected on different lines of activities. The limitation of activities in a certain people is not the consequence of a limitation of mentality, but merely the outcome of historical agencies and traditional training based thereon. In judging nations we are just as perverse as in judging individuals: both are judged by the average man from their outward success, while, of course, success is not necessarily caused by a higher state of mentality. The best exposure of this shallowness of judgment is to the credit of a Japanese statesman, who during the Russo-Japanese war, when his country held the attention of the world, remarked bitterly, "When we were a nation of artists you called us barbarians; now that we kill men you call us civilized."

In his third lecture Dr. Lowie discusses the relation of culture to geographical environment. Here he is particularly fortunate in the choice of his illustrations by contrasting the cultures of the Hopi and Navajo, which are thoroughly disparate despite the fact that both tribes have occupied for a long period the same part of northeastern Arizona, or by characterizing the Chukchi and Eskimo, the Bushmen and Hotten-

tot. Several interesting sidelights fall in this connection on the history of domestications. The fact that environment has a rather insignificant share in the formation of culture is patent to every one who can think objectively; nevertheless, in the face of the many pretensions made by that infertile pseudo-science, anthropogeography, it becomes our duty to antagonize its hollow dogmas on every suitable occasion. Dr. Lowie very well sums up his standpoint by saying that environment cannot explain culture because the identical environment is consistent with distinct cultures; because cultural traits persist from inertia in an unfavorable environment; because they do not develop where they would be of distinct advantage to a people; and because they may even disappear where one would least expect it on geographical principles.

Psychology, racial differences, geographical environment, are all inadequate for the interpretation of cultural phenomena. Culture is a thing *sui generis* which can be explained only in terms of itself. With this motto in mind, the author finally attacks in his fourth chapter the determinants of culture. He points out the great importance of cultural diffusion and assimilation, as, for instance, in the propagation of maize, the adoption on the part of the white settlers of the entire complex of aboriginal maize cultivation (tobacco and tea are as striking examples), the early traffic in bronze and amber, and the close connection and interrelations of the ancient Asiatic civilizations. Contact of peoples is thus an extraordinary promoter of cultural development. The theory of cultural evolution, to my mind the most inane, sterile, and pernicious theory ever conceived in the history of science (a cheap toy for the amusement of big children), is duly disparaged. It is worthy of especial mention that Dr. Lowie (p. 89) recedes to some extent from his former standpoint taken on the subject of convergencies. The majority of these now appear to him not genuine, but false analogies due to our throwing together diverse facts from ignorance of their true nature. I hope the author will discuss this point somewhat more fully in the near future for the benefit of his fellow-workers. What he has to say about survivals, rationalistic explanations, and the determinants of culture in general, belongs to the best portion of the book and will hardly provoke any contradiction. Culture cannot be forced into the straitjacket of any theory whatever it may be, nor can it be reduced to chemical or mathematical formulas. As nature has no laws, so culture has none. It is as vast and as free as the ocean, throwing its waves and currents in all directions. It is absurd to seek the origin of civilization in any particular region or to trace it to a single nation. In its present aspects, culture is

the common good of mankind, the product of human thought of all ages. It is something above or below the nations, but not a thing of their own. There is no nation (inclusive of primitive peoples) that has a culture entirely evolved from its own resources; there is no living nation that has a ghost of a claim to the generation of any fundamentals of culture. Our present and our future lie in our past. All that the practical investigator can hope for, at least for the present, is to study each cultural phenomenon as exactly as possible in its geographical distribution, its historical development, and its relation or association with other kindred ideas. The more theories will be smashed, the more new facts will be established, the better for the progress of our science.

The last chapter of Dr. Lowie's work is taken up by a thorough discussion of kinship terminologies, added as a concrete illustration of the methods propounded in the four preceding lectures. This subject is particularly his own domain, and as is well known, has elicited from his pen many contributions of permanent value.

B. LAUFER.

Organic Evolution. A Text-book. RICHARD SWANN LULL. The Macmillan Company: New York, 1917. XVIII, 729 pp., XXX plates, 253 figures. Price $3.00 net.

The addition of another book to the extensive literature on evolutionary biology is justified by the fact that the author is a paleontologist. Of late, paleontology has fallen into disfavor with experimental biologists and students of heredity. The reason for this is, that paleontologists, seeing at every hand animals in complete agreement with their environment, have shown a tendency to describe the evolution of different organisms in terms which, though they cannot be called Lamarckian, still savor of the Lamarckian theory. Dr. Lull states his attitude as follows:

> The geologic changes and the pulse of life stand to each other in the relation of cause and effect. This statement does not, however, imply the acceptance of the Lamarckian factor any more than that of natural selection, for whether the influence of a changing environment acts directly upon the creature's body, or indirectly through induced habit, or, whether it merely sets a standard to which animals must conform if they would survive, matters not; the fundamental principle remains that changing environmental conditions stimulate the sluggish evolutionary stream to quickened movement.

As the title implies, the scope of the book is unusually comprehensive and it is necessarily somewhat of a compilation. Several chapters are

almost entirely direct quotations. The clearness of the various chapters is inversely proportional to the amount of quoted matter.

One half of the book deals with the principles of biology. A separate chapter deals in turn with classification of organisms, distribution in space, both geographical and vertical, and distribution in time. The second part deals with the mechanism of evolution: natural, sexual and artificial selection, variation, mutation, heredity, acquired characters, etc. The remainder of the book deals with the evidences of evolution. After discussions on coloration, mimicry, parasitism and adaptation to various modes of life the author takes up in order three great lines of evolution culminating in the cephalopods among mollusca, the insects among arthropods, and mammals among vertebrates.

Doctor Lull has been very successful in preventing a book with such range from becoming a mere outline. Few books present such a wealth of material in such a readable form. The author's distinct contribution to the book, aside from his remarkable synthetic treatment of the whole subject, is his choice of examples illustrating the various phases of the subject. The animals named are, for the most part, the more common and well-known forms. The less familiar and fossil forms are shown in an adequate number of plates and figures which are a feature of the book.

The sections dealing with man are somewhat after the style of Huxley. The author's explanation of the loss of hair on the human body as being due to the use of clothing is perhaps not convincing in view of the fact that among modern men those least burdened with clothes are most glabrous. Nor, since the work of Von Luschan, Fishberg and others, do we think the "purity of the Hebrew race" is a very good example of the effect of segregation on natural selection. But errors of fact are very rare and the deductions sound and logical.

The illustrations and well-selected bibliography make it valuable as a text-book, especially in those schools which do not have access to the actual paleontological specimens. The clear and interesting presentation also adapts it to the general reader. No one working in the field of biology can ignore the findings of paleontology and the significance of their bearing on evolution. As the author justly claims, the final proofs of the evolutionary theory rest upon the documentary evidence which, in this instance, paleontology alone can furnish. It is to be hoped that this book will do much towards raising paleontology to its legitimate place among the biological sciences.

LOUIS R. SULLIVAN

NORTH AMERICA

Zuñi Kin and Clan. A. L. KROEBER. (Anthropological Papers of the American Museum of Natural History, vol. XVIII, pt. II, pp. 39–204, 3 text figures and 9 maps.) New York, 1917.

This paper opens a new chapter in the scientific study of American Indian society, and constitutes a first installment toward the filling of a long-felt need. With few exceptions previous discussions of the social constitutions of Indian tribes have merely recorded the existence of such and such social features, the number and names of clans, the terms of relationship, the number and names of fraternities, and so on. Here at last we have a quantitative study of these features, a consistent endeavor to place the exact significance of each element in the lives of the collective body. It is true that numerous elaborate studies of the rituals and secret societies have been published but these have usually been given from the native point of view, the place which they occupy in theory, not that which they occupy in fact. Thus in many cases we have, not a statement of what the social and ceremonial organization of the tribe is, but of what it ought to be, a condition perhaps never attained. Of course the theoretical view is a valuable one if it is correctly represented but our understanding of the entire organism of the tribe can never be complete until we can compare it with its actual expression in the lives of the people.

Take clans for instance. Many field workers have been content with a mere enumeration of the number of clans, perhaps distinguishing only those known to be extinct. Sometimes the part played by these in the ceremonies is added, but usually this is rather from the point of view of the ceremony than from that of the clan. The quantitative method considers much more than this. It determines, not merely the numbers and names, but the relative strength in individuals, the geographical distribution, relative ceremonial value, and so on. For instance, how illuminating it would be if we had a complete gentile census of the Chippewa Indians, band by band, so that we could form a mental map containing this data! Such mapping is similar to that made familiar by the investigations of population now so popular among social workers, though the social worker's mind is rather on the future and the ethnologist's on the past.

Professor Kroeber's paper is essentially a social analysis of the kind indicated. Although embodying mainly a record of facts it nevertheless expresses by its very arrangement a theory — or perhaps I should rather

say view — to which its author has been led by his own investigation. This view is well summarized in his introduction and indeed in the opening sentences,

> The foundation of Zuñi society is the family. Life centers about the house. The clan is above all a ceremonial institution.

And what follows is simply an elaboration of these statements. In contravention of those speculators who postulate the abnormal as a precursor of the normal, and hang upon the seemingly unusual elements of aboriginal life, such as the clan and moiety, a fantastic psychology, Professor Kroeber shows us a mental attitude toward family, friends, and neighbors, recognizable as human, one which we can understand. He says:

> With the view that the present state of Zuñi society is an altered one, and that it was preceded by a condition of the predominance of clan over family, it is . . . vain to quarrel. If any one finds it more profitable to demonstrate that such and such must have been the practices of this or that or all nations at some time before we have cognizance of them, rather than to understand and weigh in just balance their manners within the historic period, that satisfaction cannot be denied him: provided he does not proclaim or assume that the rearing of such hypothetical dogmas is the justifying purpose, or the ultimate goal, of ethnology and history. Yet, it is also justifiable that those not infected with such theories should exact much and specific evidence before inclining any favor to the view that the fundamental organization of the society of the Zuñi and similarly constituted peoples was once on a clan basis.

In accordance with his conclusion as to the fundamental nature of kinship he deals with this first, giving a detailed account, illustrated by an actual genealogy, of the various terms of relationship. This is followed by a short section on "The House and Marriage", which accentuates feminine ownership of the house as the central point of the Zuñi matriarchate, but at the same time calls attention to the fact that in consequence of this,

> the position of woman is not materially different from that which she occupies in nations of non-matriarchal institutions,

and finally, — evidently in answer to those theorizers who associate a clan organization with group marriage, — he emphasizes the essentially monogamous character of Zuñi marriages.

The third, and by far the longest section deals with clans, which Professor Kroeber examines from every possible point of view, their number and names, connection with marriage regulations, association

into phratries and so forth, subdivisions, size, internal government, religious and governmental functions, together with a general estimate of the entire Pueblo clan system. In the last section is a description of the present condition of Zuñi town as illustrated by an excellent map from a survey made for the author in 1915 by Mr. Mark Bushman of Gallup. The changes undergone by the structures generally and the religious structures in particular are here discussed and finally there is an examination into the location of the town of Hallonawa which is mentioned in early Spanish narratives; this Professor Kroeber concludes to have been, not the place now so called on the south side of the river, but at or near where Zuñi is today.

Too much praise can hardly be given to the idea embodied in this work or to the general method in which it has been executed. Doubtless students better acquainted with the Pueblo region than the reviewer would find details to criticize, but as a whole we have a solid and important contribution to the study of aboriginal Indian society.

In one particular, however, it appears to the reviewer that some injustice has been committed by Professor Kroeber, namely in his treatment of the evolution of the Hopi state as presented by Dr. Fewkes. It is true that a common source of danger is the tendency to assume from native tradition or native custom in the present a past social condition which is purely hypothetical; still it is going quite too far to conclude that such data can give absolutely no valuable hints as to past conditions. If ethnology is of no assistance in the reconstruction of past conditions, it is reduced to the level of a description of more or less curious and amusing phenomena, or else to a study of social psychology. I proceed to a discussion of this point.

In the first place Professor Kroeber attributes to Dr. Fewkes the theory that each Hopi clan is descended from a former village community, with which incidentally what later became the clan ritual was associated as a community ritual (p. 142). What Dr. Fewkes actually does claim on the basis of native tradition and other evidence, is that the Hopi grew up as the result of a union of different *groups of clans*, the three most important of which came from the north, south, and east respectively. Secondly, Professor Kroeber's critical comparison of Pueblo clans has shown him that practically the same system of clans is found throughout all of Pueblodom, which he thinks would not have been the case had certain clans come to Hopi from one quarter and certain from another. If this similarity in number and names of clans, agreeing with well-known similarities in culture, were accompanied by a

similarity in language, Professor Kroeber's argument would be decidedly strong. But if the immigration of a set of clans from the Paiute country be disproved by the fact that the same clans are found on the Rio Grande, how does Professor Kroeber account for the fact that Hopi is plainly a Shoshonean dialect, and that the remainder of the Pueblo Indians are divided among three stocks quite distinct from Shoshonean? Plainly we must suppose that from one side or the other, probably from both, there has been a diffusion of clans. If at the time of their immigration the Hopi did not have clans, we may grant that clans may have been superposed upon them in such a manner that none of the original elements could afterward be discriminated, but this is as speculative as anything Dr. Fewkes proposes. The facts to be explained are these. The Hopi are a people having a Pueblo culture like the Zuñi, Keres, and Tewa peoples, but a Shoshonean language unlike any of them, and on the other hand they are the only people employing a Shoshonean dialect to have such a culture. Do not these facts at once suggest that the condition has been brought about by intimate contact between one people which had the culture and another which had the language? And when a native tradition is found which seems to square with the expected in these very particulars, why should we not consider it a more probable explanation than to suppose that a single body of Shoshoneans had been violently Puebloized in culture or a single body of Pueblo Indians Shoshonized in language, suppositions which have not even the merit of being *equally* plausible? And if we find native tradition agreeing with the expected in one particular, why not assume its correctness in the threefold origin of these people?

The fact that the same clan names are found throughout Pueblo territory is no insuperable obstacle to such a supposition. In the first place the number of names likely to be selected to designate clans is not unlimited and we may confidently predict independent origins for some of them, but, more than that, the removal of one woman to another town transplants the clan along with her and no very great period of time need be required for all of the clans in a given totemic area to spread throughout that area. Finally there exists such a thing as assimilation of clans through which clans of different names in different towns will consider themselves related and in time change, or exchange, their names. Thus each of the seven Natchez clans is today identified with a corresponding clan among the Cherokee although not half of them bear identical names.

But there is something besides. Professor Kroeber, in order to

make his point, relies on purely qualitative data, the presence of clans bearing identical names throughout the entire Pueblo area. It is, however, vital to the discussion to see in what relative numerical proportions these clans occur. Unfortunately we can not do this for all of the Pueblo towns but we can do it in the case of a few of them, the Zuñi, the Hopi pueblos, and Cochiti. On page 149 Professor Kroeber has himself given a numerical comparison of the principal clans in these towns, the true figures being altered just sufficiently to yield a fair comparison between them. He has not, however, separated the two Hopi pueblos of the east Mesa, Walpi and Sichumovi, which it is important to keep apart, because, according to Hopi tradition, Sichumovi was a Zuñi colony. In the subjoined table I have made this alteration and I have made only three entries, one for each of the groups of clans supposed to have come to Hopi from the north, south, and east respectively. I have used the figures upon which Kroeber depends, merely increasing them proportionately the better to make a direct comparison. I have put all of the Hopi pueblos in one column except Hano as being of Tewan extraction and Sichumovi.

	Hopi	Sichumovi	Hano	Zuñi	Cochiti
Clans from North.....	61	—	—	20	—
" " South.....	507	616	892	340	303
" " East.....	1068	1022	747	1280	1336
	1636	1638	1639	1640	1639

If there is anything in the traditions under consideration we ought to find those clans from the north best represented comparatively in Hopi, most poorly represented in Cochiti, the farthest east of these pueblos, and poorly represented in Hano because it was a pueblo of eastern origin and in Sichumovi because it is supposed to have come from Zuñi. And in fact it is best represented among the true Hopi and not represented at all in the other pueblos mentioned. It is true that there are a few of this group at Zuñi but as between Zuñi and Cochiti this is what was to have been expected. It is true that we ought also to have expected better representation at Hano and Sichumovi; a hundred per cent. confirmation is not, however, to be looked for. Again, as between the clans from the south and east we ought to expect the representation of southern clans greatest among the Hopi and poorest at Cochiti, and—as between the Hopi, Zuñi and Cochiti—this is precisely the case. Why southern clans appear to be represented better still at

Sichumovi and Hano I do not know, but the populations of these two towns are so small that a slight increase or decrease on one side would result in upsetting the proportions very considerably.

I do not consider these facts to have disposed of this question by any means. If we could have a clan census from every other pueblo it is likely that a very different condition of affairs would be disclosed. Yet, admitting the whole question to be provisional, I feel as much justified in accepting Hopi tradition as embodying a true kernel of fact as Professor Kroeber in rejecting it because clans are qualitatively present throughout the Pueblo area. All of this shows that we need much more information from the remaining pueblos, and more studies of the kind here under consideration.

JOHN R. SWANTON.

Zuñi Kin and Clan. A. L. KROEBER. (Anthropological Papers of the American Museum of Natural History, vol. XVIII, pt. II, pp. 39–204, 3 text figures and 9 maps.) New York, 1917.

Dr. Kroeber in his admirable monograph on "Zuñi Kin and Clan" throws down the gauntlet to the partisans of the theory of the clan as the basis of society. The challenge at large is no doubt called for, and to Dr. Kroeber's appreciation of the importance of the blood kin compared with the clan and of the subordination of clan to tribe I take no exception; nevertheless I incline to think that Dr. Kroeber has misinterpreted or undervalued in theory, even on his own scrupulous array of data, the position of the clan in Zuñi. He has carried through this theoretical undervaluation along three lines, first by separating too rigidly the ceremonial functions in Zuñi culture from what he calls social functions, and limiting clan participation to the former; second, by overlooking, in comparison with his otherwise careful research, a number of economic as well as of ceremonial facts which may have considerable significance in clanship analysis; third, by leaving out of consideration the more or less conscious attitude of the Zuñi themselves towards clanship. That a woman should be reprimanded by her husband for having spoken bitterly of a clansman, reprimanded merely because the man was her clansman, an incident once described to me, may be potentially as significant a fact as the fact of one national criticizing another for abusing a fellow countryman. Given such a sense either of nationality or of clanship it can hardly be said that "the clan [or the nation] is not thought of in ordinary personal relations of man to man, or of man to woman" (I fail to see how the fact of clan exogamy does

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not enter daily into the personal relationships of man to woman), and only by taking into account the psychology of the clan sense of solidarity could such extraordinary facts as migration by clan be understood. Would Dr. Kroeber say that he observed at Zuñi no expressions of clan solidarity sufficient to account for any such migration by households unrelated by blood as are reputed to have occurred for example from Zuñi to the Hopi or from Laguna to Isleta? I realize that it has still to be proved that such households were unrelated by blood or even that clanship was a selective principle among the immigrants.

Zuñi clans play an important part, as Dr. Kroeber admits, in Zuñi ceremonialism, but if ceremonialism is itself, as it undeniably is at Zuñi, an essential of both public and private economy, how can it be said that clanship is "only an ornamental excrescence upon Zuñi society"? To say that without the clans the forms of Zuñi religion will have to be made over in part, "but the life and work of day to day, the contact of person with person, will go on unaltered," is to the student of Zuñi ceremonialism much like saying that however men express thought or feeling, they breathe and digest, they are born and they die.

A more persuasive content would have attached to Dr. Kroeber's thesis in regard to the clanlessness of daily life had he shown in a number of families or even in his one pedigreed family the amount of family intercourse and cooperation which occurs, to what extent related households work together in agriculture—in planting, in harvesting and in threshing, in sheep-raising and dipping, what relations cut feather-sticks for others, or have attended at childbirth or at death or burial, or what relatives have adopted children or given a roof to the discontented at home. All of these economic or ceremonial activities he would have found performed, I have no doubt, by kin and not by clan, but he would have had to consider whether, kin lacking, any of them would have been undertaken by clan. The preparation of a corpse and its burial are undertaken by the paternal kin, but supposing paternal kin are lacking, would not the paternal clanswomen and men of the deceased render the requisite offices rather than his maternal kin or his own children?

And is Dr. Kroeber certain that in planting feather-sticks certain *koko* or gods are not viewed as clan ancestors, or that in the building or repair of the *shalako* houses, in supplying them with wood, and in the field work of the *shalako* hosts, clanspeople of the hosts or impersonators in the ceremonial, mere clanspeople, do not help? In the repair of the house of the ranking priesthood Stevenson states explicitly (*The Zuñi Indians*, p. 227) that the work was done by the clanspeople of the priests,

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the workmen appointed by the *pekwin* or sun priest, a member of the same clan as the aforesaid priests. The names of the appointees were called out by one of the *tenientes* or council. Clearly here was a case of communal labor by clan, ceremonial, if you like, as well as economic; but it may well be a ceremonial survival of an early provision of merely economic clan labor.

"Clan and kin are distinct things, one rather lightly superimposed upon the other," concludes Dr. Kroeber. We would run less risk of the very schematization Dr. Kroeber criticizes elsewhere, it seems to me, were we to figure the relation between kin and clan as a circle with the kin at the center and the clan at the circumference, letting the radii represent the functions, ceremonial or "social." When these functions can be performed by a few persons the kin, as long as kin are available, are called upon; but when the kin are unavailable or when the services of a comparatively large number are desired clanspeople or the clan as a whole are called upon. Take the clan relation towards the *koyemshi*. The final day of the *shalako* ceremonial the head of each *koyemshi* has to be washed and his back loaded with presents. Members of his father's clan nearest to him in blood perform these functions. But all his father's clansmen have cut feather-sticks for him and male and female representatives from the households of his father's clan march into the plaza bringing him gifts of meal or bread, of melons, of mutton and store-bought goods. Here is one occasion at least, Dr. Kroeber to the contrary, where the clan does act as a whole.

To ensure the clan gifts to the *koyemshi* messengers have gone from house to house. These messengers belong to the household of the *koyemshi's* father's sister, his *kuku* par excellence, I think, I am not sure. The alternative is that the messengers belong to a household having particular clan authority or leadership. Dr. Kroeber would summarily dismiss this alternative with the statement that there are no clan heads. I have heard too many references to the selection of ceremonial impersonators by clan heads to be satisfied with Dr. Kroeber's conclusion in general on this point without further investigation. Indeed the whole subject of the selection of persons for ceremonial rôles belonging to or associated with clans needs the most careful study. Take the present confusion of our knowledge about the impersonation of *pautiwa*, *koko awan mosi*, the gods their director. Stevenson states that *pautiwa* is chosen by the Corn clan and the children of the Corn clan (persons whose fathers are Corn clansmen) assembled in the house of the head of the clan. Dr. Kroeber states that *pautiwa* must be a Dogwood clans-

man and selection by the clan as a body he disputes as unparalleled in Zuñi practice. According to my informants *pautiwa itiwonna* (*pautiwa* of the winter solstice ceremonial) must be a Dogwood although the two men of whom one or the other now habitually impersonates *pautiwa* were sons of a Turkey clansman who in his day impersonated *pautiwa* and who *handed the position down to his sons*. This Turkey clansman was, however, child of the Dogwood. Unfortunately my notes leave a doubt as to whether the present impersonators are Dogwood. If they are their fathers must have taken the rather unusual step of marrying into his father's clan. In spite of these facts my informants stated that the impersonator of *pautiwa* would be selected by the *pichikwe amosi*, the directors or heads of the Dogwood people.—But *pautiwa itiwonna* is not the only impersonation of *pautiwa*, there is *pautiwa molawia* (the impersonation in the *molawia* ceremony at the close of *shalako*). The impersonator of *pautiwa molawia* is or should be a Tobacco clansman selected by the heads of the Tansy-mustard clan, by the same "heads," I surmise, who choose the girls who run in the *molawia*.—Rather complicated!

Another point in regard to clan heads. Is it not possible that the clan *ettowe* or fetiches listed by Dr. Kroeber convey a certain distinction or headship to the families in charge of them if merely in accordance with the conceptual schematization Dr. Kroeber points out so well as characteristic of the Zuñi? The argument that the possession of *ettowe* by the clans, however, is evidence "that the Zuñi clan is much more a part of a ritualistic scheme than a body of kindred" is to my mind not valid, for may not the *ettone* be merely a pattern which is associated with group formation, with the formation of any group? Incidentally I would suggest that the *ettone* is property, quite as much property as the "good will" of an American business or the saint's image in a Catholic church (I surmise that the Zuñi think of their *santu* as an *ettone*. As for the canes of the *tenientes* I venture to say that they too are approximations of *ettowe*), and in so far as the clans are possessed of *ettowe* it can hardly be said that there is nothing "that can be considered clan property."

It is from lack of data that Dr. Kroeber disregards the possible significance of the clan *ettowe*. His disregard of the question of leadership among the clans is more wilful. In view of the fact that the ranking priests of Zuñi must belong to the Dogwood clan (likewise as I have said, the personator of the ranking figure among the *koko* or *kachina*), it is difficult not to recognize a degree of clan supremacy and not merely

clan equivalence at present in Zuñi or in view of the clan autocracy at Acoma, for example, not to speculate as to a still greater degree of clan preponderance at Zuñi in the past. Take the matter of communal service for the ranking priesthood. In Acoma four sacred rabbit hunts are held yearly and the game given to the paramount priest; his land is also planted for him by the people and it is he or his "uncles" or "brothers" who assign farm lands to the people. Nowadays in Zuñi only one sacred rabbit hunt is held every four years (the game goes to the ranking priesthood) and there is neither planting of priest-owned fields by the people nor land allotment by the priests, but may not the priestly prerogatives and the priestly control have once been larger—before Spanish interference? At least the question needs investigation.[1]

Most painstaking and accurate are Dr. Kroeber's data in kinship terminology but here again because of his clan prejudice his comment or inference is sometimes open to question. Is not the Zuñi indifferent to fine discriminations of consanguinity and uninsistent upon carrying relationship back very far not because he is merely linguistically contradictory or merely uninterested in genealogy but because of his clan consciousness? It is just as you might answer when asked of one John Smith, a seventh or eleventh cousin, "Oh I don't know exactly how he is related, but he is one of that Smith family." Given the closed circle of clanship, fine distinctions are not urgent, only gross distinctions, such as my own mother, my own father, my blood relatives, are in order.

Towards the evidence kinship terminology may have for relationships not found today at Zuñi it seems to me that Dr. Kroeber is somewhat impatient. The similarity of terms for a woman's brother's son and for son-in-law, is, as Dr. Kroeber says, a striking argument for cross-cousin marriage nor is Dr. Kroeber's preferred explanation incompatible, as far as I can see, with the argument. Cross-cousin marriage certainly does not exist today in Zuñi nor does marriage with a deceased brother's wife and yet I would not close the case for cross-cousin marriage in the past or, if the Zuñi call a father's brother's wife *inniha*, stepmother (that so careful an observer as Dr. Kroeber did not get this application of the stepmother term *inniha* would make me doubt

[1] Judging from a recent land dispute, sacerdotal land control would not seem foreign even today to the Zuñi point of view. Kwanton, an ex-governor, holds a Spanish deed to land to which in 1916, if not before, he was setting up a claim. The *tenientes* refused to consider the claim, the lieutenant-governor throwing down his cane in the plaza in the course of the quarrel. As a result of this controversy as well as of other matters in dispute—the *tenientes* were changed by the ranking priests and Kwanton's party, as we would say, recognized.

the accuracy of my informants except that Dr. Kroeber himself found some puzzling applications of the term), for the levirate merely because the Zuñi themselves repudiate and scorn the idea of the practices. The Zuñi are not only thorough believers, as Dr. Kroeber has pointed out, that whatever is, is right, but that whatever is not, is wrong. In suspending conclusions on early marriage customs, I might even wonder what Zuñi women mean when they refer to *shalako* night as a time "for stealing a word with our other husbands" were I not afraid of being derided by Dr. Kroeber as one fascinated by the theory of promiscuity.

Nevertheless I have courage enough to hold that "the marked tendency towards descent from father to son in the priesthood" is of interest—both psychologically and historically. Dr. Kroeber suggests an interesting psychological basis for the clan, namely that a woman is *felt* to be a very different thing from a man in relationship. Would he not be willing to entertain the hypothesis that paternal descent becomes a principle in ceremonialism because a man is felt to be a very different thing from a woman in religion? From the historical standpoint, that patrilinear kinship tends to be associated with Zuñi ceremonialism may not be an unimportant fact at all. Suppose, for example, that it becomes established that part of the complexity of Zuñi ceremonialism is due to the incorporation of Catholic rituals, would not the presence of the principle of paternal descent in a ceremonial be at least one clue as to whether in other particulars Spanish acculturation was to be looked for?

As a parting shot on the interpretation of Zuñi clanship let me ask Dr. Kroeber why if it is true that to the native mind the clan is "essentially a schematic subdivision, and perhaps a more or less artificial one, of the community as a whole," why if it is true that the clan is "a ceremonial rather than a socially functioning body" (a misleading distinction as I have said before), then why are the Zuñi so conspicuously ready to talk, as Dr. Kroeber truly remarks, about their clan affiliations, reticent as they are about all ceremonial matters (notice that about blood kinship they are not reticent either) and why, seeing that a Zuñi will change from fraternity to fraternity and even from *kiwitsine* to *kiwitsine*, and that he may drop out of his priesthood, why does he never change from one clan into another or ever dream of dropping out of the clan he was born into? A Zuñi may be taken at any time of life into a household other than that he was born to, adopting the kinship terms and attitudes proper to his status in the adoptive household, and a Zuñi may go to live in another tribe. How deeply he may feel the effects

of either the tribal or the household transplantation we do not know, but one thing we do know, neither tribal nor household adoption has any effect upon clanship. How does clanship alike in native theory and practice come to be the most *fixed* character of the whole social organization?

ELSIE CLEWS PARSONS

ASIA

The Foote Collection of Indian Prehistoric and Protohistoric Antiquities. Madras Government Museum. Catalogue raisonné. ROBERT BRUCE FOOTE. Government Press: Madras, 1914. 262 pp.

The author of this catalogue, Mr. Foote, who died in 1912 at the age of seventy-eight, was a geologist, who joined the Geological Survey of India in 1858 and retired as senior superintendent of the service thirty-three years later. The greater part of his service was spent in southern India, where he accomplished much in researches of the geology and palaeontology of the country. In 1863 he succeeded in discovering palaeolithic implements near Madras, and thus became the pioneer of prehistoric archaeology in that region and subsequently a recognized authority. His extensive and prolonged journeyings in southern India enabled him to accumulate an extensive collection of objects found chiefly in the Madras Presidency and the neighboring states of Mysore and Hyderabad, also in Baroda and other parts of northern India. The entire collection was purchased by the Madras government in 1904, and a special room was built for its reception in the Art and Ethnological Section of the Madras Museum. The author did not live to see his catalogue out, but had revised most of the proofs. The work, as stated in the preface, has been brought out in two volumes, the second to contain notes on the ages and distribution of the antiquities, plates, map, and index. This second volume has not been sent to me for review or is not yet out. The present first volume (not thus designated on the title-page) contains only a descriptive list of the objects in the collection, arranged according to the districts and localities in which they were found. Altogether 4135 specimens are listed, in addition 68 from Ceylon, 205 beads, and 128 bangles and glass frits, these groups being numbered separately. The descriptions are the briefest possible, only form, material, color, and locality being noted. With the exception of a few cases, no information is given regarding the circumstances of the finds, which would be of especial importance with reference to the glass objects, as the time for the introduction of glass into India has not yet

been determined. Measurements are not added. We hope to come back to this work when the second volume is available.

B. LAUFER

Catalogue of the Prehistoric Antiquities from Ādichanallūr and Perumbāir. Madras Government Museum. ALEXANDER REA. Madras, 1915. 50 pp. 13 plates in half-tone.

This catalogue offers a description of the important finds made in the burial ground at Ādichanallūr (the southernmost district of the Madras Presidency), covering an area of one hundred and fourteen acres and the most extensive yet discovered in southern India. The excavations were conducted by the author of the catalogue at intervals from 1899 to 1905. These graves are characterized by funeral urns deposited in pits, which were excavated in the solid rock or in the gravelly soil. On the whole, they are similar to those found in other graves of southern India. The pottery is unglazed, but exhibits a red and black polish, and almost lacks decoration. Of greater interest are the objects of metal; chiefly iron, bronze, and gold. The only gold objects are oval diadems of a thin, flimsy gold leaf, supposed to have been tied around the forehead of people of rank and to be substitutes of a more substantial piece of jewelry, as worn in life. Of iron, many implements and weapons like swords, daggers, tridents, hatchets, spearheads, and arrowheads, were found, always placed point downward, as if they had been thrust into the surrounding earth by the attendant mourners. No implements or weapons are made of bronze, all articles of this alloy being vessels of various shapes, personal ornaments, such as rings, bangles, and bracelets, or ornaments attached to the bases and covers of vases, such as buffalo with curved horns. Iron, then, served for the implements of everyday life; bronze was the material for artistic expression. The domestic animals represented in bronze in full figures are buffalo, dog, sheep, and rooster; the wild animals are tiger, antelope, and elephant. The animals are usually arranged in circular rows on a complicated metal framework. Although of crude workmanship, they can be readily identified. A dog is represented with a great deal of realism and motion. The cow and other animals distinctive of Indo-Aryan culture are absent. The bronze alloy consists of copper 75 per cent., tin 23 per cent., lead 0.2 per cent., iron 0.4 per cent. The bronzes, it is said, exhibit a high degree of technical skill and manipulation of the metal. On several of the bronzes are traces of textiles, preserved by contact with the oxidized metal. Many of the vessels contained rice and millet seeds. In a

number of urns there were fragments of mica in pieces about an inch in length.

The second and smaller collection included in this catalogue bears on prehistoric remains from the neighborhood of Perumbāir, about fifty miles south of Madras, excavated by Mr. Rea from 1904 to 1908. Here the ancient burial sites are indicated on the surface by circles of rough stone bowlders; and in the center of each circle, at a depth of from two to seven feet, was found either a pyriform urn or an earthenware cist, covered with a dome-shaped lid, and posed on three rows of short legs. In and near the graves were found pottery, stone implements for grinding, a few iron objects, and some chank-shell ornaments.

The objects illustrated on the thirteen plates are reproduced on too small a scale (on one plate as many as forty-one pieces of pottery are arranged), and do not allow the study of details. Measurements are given in inches.

B. LAUFER

The Mythology of All Races. Vol. VI: *Indian.* A. B. KEITH. *Iranian.* A. J. CARNOY. Marshall Jones Co.: Boston, 1917. Pp. IX, 404, 5 figs., 44 pls. Price $6.00.

Keith and Carnoy are to be heartily congratulated on these first attempts at a historical and synthetic treatment of Indian and Iranian mythology. Carnoy's account is the first of its kind. For Indian mythology there are a few adequate works on special subjects such as Macdonell's *Vedic Mythology*, Hopkins' *Epic Mythology*, Grünwedel's *Mythologie des Buddhismus in Thibet und der Mongolei* and *Buddhistische Kunst in Indien* (translated by Burgess); but there is no general historical treatment. Both accounts adequately fulfill their purpose as popular summaries; both will also be of value to specialists. Keith's judgment is sober and cautious; Carnoy is more speculative and venturesome in the projection of facts into theories.

Keith devotes two chapters to the Rig-Veda, one to the Brāhmaṇas, two to the Epic, one to the Purāṇas, one to Buddhism, one to Jainism, and one to Modern Hinduism. Carnoy divides his material into the treatment of the wars of gods and demons, of myths of creation, of the primeval heroes, of legends of Yima, of traditions of the kings and Zoroaster, of the life to come.

Keith keeps strictly to the main line of mythology and offers little on the development of myth to legend, folklore, and traditional history. Carnoy devotes much space to the latter development in the Persian

epic. In India the mythological material is so vast, the historical background so obscure, and so little of the preliminary work of analysis and classification has been done that no well-rounded treatment of the subject as a whole is possible at present. Much of the material of the so-called Indian mythology is Dravidian rather than Aryan, but it is impossible at present to distinguish Aryan from Dravidian with any certainty.

The most satisfactory chapters of Keith are those on the Rig-Veda, the Brāhmaṇas, and the Epic. Here he is completely master of his material. The mythology of the Purāṇas, of Modern Hinduism, and of Jainism is sketched cursorily in only the broadest and most general outlines. The chapter on Buddhism is the least satisfactory of all. Here Keith's touch is not so firm and sure as in the earlier chapters. Keith argues (p. 188) that "we have no assurance that a single Buddhist text which has come down to us is even as early as two hundred years after Gotama had departed." So much may be granted, but if this assumption is made it is not fair to claim that we have "the authority of the Buddha himself for his abode in the Tuṣita heaven and his descent from it" (p. 194) or to speak of "the Buddha's belief in his own superhuman nature" (p. 193) and "the natural explanation that the Buddha, like his followers, regarded himself as really divine" (p. 195). In India myths and legends develop with amazing rapidity, and two hundred years are not to be dismissed lightly. If no text can be assigned with certainty to the period within two hundred years after Buddha's death there is no certainty that the important mythological matters ascribed in the Pali texts to Buddha himself can be earlier than two hundred years after his death. This is admitted by Keith himself on p. 188: "for we have not, and never can expect to have, any conclusive proof as to the actual views and teachings of Gotama." The contention on p. 189 that "it was clearly more easy for a preacher of faith in a personal god to become regarded as himself a god than to deify a man who *ex hypothesi* was no god and had no real belief in the gods" may be true of the Anglo-Saxon mind, but is, I think, utterly fallacious in the case of the Hindu mind. In the dispute between those who approach Buddhism from the point of view of the Hīnayāna and the Mahāyāna texts respectively one thing is certain, that the Pali texts represent the ideas of only one Buddhist sect. Different groups of monks interpreted the enigmatical teachings of the Buddha according to their own thoughts and feelings. The Pali canon does not give us the unified tradition of Buddhism before the early split into sects, although it preserves much

that is at variance with the professed doctrines of the Cinghalese school. Much that is put into the mouth of the Buddha may be due to speculative accretion generations after his death. The "thus I have heard" is no more proof of originality than is the corresponding formula of the Mahāyāna texts. Further, many elements in the life of the Buddha himself (if any of the traditions concerning his life and deeds go back to real memories of his followers) show that he himself lived a life of ministry nearer in many ways to the Bodhisattva ideal of the Mahāyāna than to the Arhat ideal of the Hīnayāna.

Both Keith and Carnoy (pp. 5, 25, 30, 263) refer to the names A-ru-na-aš-ši-il (or U-ru-w-na-aš-ši-el), In-dar (or In-da-ra), Mi-it-ra-aš-ši-il, and Na-ša-at-ti-ia-an-na found in the Hittite records at Boghazköi as Indo-Iranian. The reviewer has tried to show in a recent article in *The American Journal of Semitic Languages and Literatures*, 1917, pp. 261–82, that the matters involved are, as yet, purely linguistic ones on which no historical conclusions should be based.

Keith argues here (pp. 125 ff.), as in previous articles, that Krishna was originally a vegetation-god, become anthropomorphic. He is overskeptical of the possibility of men becoming gods in India. The classical prejudice against Euhemerism is not valid for India. In the case of Krishna it is much easier to explain the mythology as a later accretion (as in the cases of Buddha and Mahāvīra) than as fundamental. The reviewer can interpret the Krishna of the epic only as a man deified. Where among the vague anthropomorphic figures of Indian mythology is there one which has become so concretely human as the Krishna of the epic? The Krishna (son of Devakī) of the Chāndogya Upanishad 3, 17, 6 cannot be lightly dismissed as not identical with Krishna (son of Devakī) of the epic just because the identification invalidates a theory that Krishna must be a sun-god or a vegetation-god. The passage of Patañjali (p. 126 and *Zeitschrift der deutschen morgenländischen Gesellschaft* 64, p. 536) is far too late to prove that the mythological elements were original. Keith argues (*Journal of Royal Asiatic Society* 1915, p. 547) that a mere man is not sufficient to explain the mythological elements of the Krishna story. It may be answered that a mere god is not enough to explain the concrete human figure of the epic Krishna. On p. 126 Keith does not express definitely his attitude toward Garbe's very doubtful identification (*Indien und das Christentum*, pp. 215 ff. and *Archiv fur Religionswissenschaft*, 1913, p. 537) of Buddhist Rummindei (Lumbinī) with Rukminī, the name of Krishna's wife. There is no convincing proof that Krishna worship had penetrated so far east-

ward by the sixth century B.C. For philological difficulties in the way of the identification see Charpentier (*Indian Antiquary*, 1914, p. 18).

Keith (p. 177) is inclined to follow Garbe's theory that the Çvetadvīpa story is to be explained on the basis of Hindu contact with Nestorian communities in the vicinity of the Balkash Sea. Garbe (in *Indien und das Christentum*) has summed up the weighty evidence against Weber's theory that the story is to be explained as referring to Christian communities at Alexandria; Pelliot (*T'oung Pao* 1914, pp. 623–44. *Cf.* Laufer in *American Anthropologist*, 1916, pp. 572–3) has made available material which utterly invalidates Garbe's theory, at least so far as present evidence goes. There remains only the conclusion that the story is mythical (see Charpentier, *Journal Asiatique*, 1910, II, p. 605). It is most unlikely that any early Christian community could have served as the basis of such a description. The story may belong to the same development as that which resulted in the Buddhist descriptions of Sukhāvatī. It is more probable that the word *dvīpa* of Çvetadvīpa is to be connected with the mythical *dvīpas* of Hindu cosmology than with any actual island.

In connection with the story of the Bhaviṣya Purāṇa (p. 183) concerning the "Magas" Keith might have referred to the important collection of material made by Spooner ("The Zoroastrian Period of Indian History" in *Journal of Royal Asiatic Society*, 1915, pp. 63 ff., 405 ff.). Spooner's theories are far more pretentious and wide-reaching than the facts warrant, but the article should stimulate a closer consideration of the possibilities of early intercourse between Persia and India. Jackson's "Notes on Allusions to Ancient India in Pahlavī Literature and Firdausi's Shāh Nāmah" (in *Festschrift Windisch*, p. 209) seems to have escaped notice in the bibliographies of both Keith and Carnoy.

It is by no means certain that the starting point for the personification of Vāc was the thunder (p. 53). That is one of Yāska's guesses not fully corroborated by the evidence of the Rig-Veda itself. Vāc is probably the personification of the voice (spoken word or hymn) which, as the sacrifice assumed a cosmic aspect became cosmic too.

The bibliography, though well chosen, is, of course, far from complete. I would add at least such titles as Dowson, *A Classical Dictionary of Hindu Mythology* and Garrett, *A Classical Dictionary of India*—helpful, if not useful for scholarly purposes; O. E. Martin, *The Gods of India* (Dent, London, 1914)—an uncritical but very useful popular account; Vans Kennedy, *Researches into the Nature and Affinity of Ancient Hindu Mythology*—old, but giving many translations from the

Purāṇas not to be found elsewhere; Coomaraswamy, *Buddha and the Gospel of Buddha;* H. K. Sastri, *South Indian Images of Gods and Goddesses;* H. Oldenberg, *Die Lehre der Upanishaden und die Anfänge des Buddhismus*—a work of fundamental importance for the social and intellectual background; G. Oppert, *Die Gottheiten der Inder* in *Zeitschrift für Ethnologie*, 1907, pp. 296, 501, 717. Lists are given of all the articles in the first eight volumes of Hastings, *Encyclopædia of Religion and Ethics*, bearing on Indian and Iranian mythology.

Keith maintains a wise reserve on the question of the early relations between Buddhism and Christianity. *Sub judice lis est.*

The Simurgh story as related by Carnoy (p. 289) has striking parallels with the story of Garuḍa in Mahābhārata 1, 29, 30.

Carnoy's description of Narāçaṁsa as the sacrificial fire in India (p. 284) and references to pages 44–45 is not borne out by the statement of Keith, who refers to Narāçaṁsa as "the personification of the praise of men, or possibly the flame of the southern of the three fires, which is particularly connected with the fathers." Narāçaṁsa is a very uncertain figure. There are several different theories concerning him.

Carnoy has done a great service in calling attention to many curious coincidences between Iranian myths on the one hand and Indian and Babylonian myths on the other. No definite conclûsions can be drawn, as yet, from what may be mere coincidences, but as the historical evidence becomes fuller such collections of parallels will be valuable. He deserves high praise for the success of this first effort to treat Iranian mythology historically and to bring Avestan mythology into connection with the legends of the later Persian epic.

WALTER EUGENE CLARK

SOME NEW PUBLICATIONS

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DISCUSSION AND CORRESPONDENCE.

Moccasins

In the April–June number of the *American Anthropologist* (vol. 19, pp. 297–301), Dr. B. Laufer has made some friendly and critical remarks on my paper *Moccasins and their Relation to Arctic Footwear*. I am grateful to Dr. Laufer for the interest he has taken in my work; I find it necessary, however, to make a reply to his criticism.

Dr. Laufer regrets that I do not grasp the origin and culture-historical position of other types of footwear than moccasins. He is not quite right in this statement. Among the best results yielded by my study of clothing I reckon the analysis of the Eskimo boot (*Arktiske Skinddragter*, pp. 179–194, recapitulated in *Moccasins, etc.*, pp. 201 f.) and the tracing of its evolution from a sandal and a stocking. I have been able also to prove the existence of "sandal-boots" in different parts of northern Asia and in Pamir, and my material has led me to suggest that "sandal-boots" have a wide distribution in Central Asia (*Arktiske Skindraggter*, pp. 194–96, *Moccasins, etc.*, p. 234 note and p. 236). I have not, however, made Central Asiatic footwear the special subject of any publication. In my moccasin paper I have only touched the question of Central Asiatic footwear briefly, just to give some reasons for my view, that true moccasin-forms were not characteristic of, perhaps never found in the clothing of Central Asiatic nations. I found it unnecessary to enter further upon the study of Central Asiatic footwear in my moccasin paper. It is gratifying to me that Dr. Laufer supports my view regarding the absence of connection between the moccasin-group and the footwear of Central Asia; he even states that the latter has "positively nothing to do with the moccasin." So much the better for my theory about an original connection between the moccasin and the snowshoe.

I think Dr. Laufer is right when he assumes that the riding-horse has a share in the evolution of the riding-boot; I do not see, however, that this theory affords any full evolutionary history of the "riding-boot"—it says nothing at all about the prototype of the "riding-boot." It even remains to be seen, whether all riding-boots have sprung from one prototype—this can only be proven by means of a thorough analysis of all riding-boots extant. I must confess, though, that I am rather

surprised at Dr. Laufer's disapproval of my "transformed moccasins" in the Amur country and Siberia. My "transformed moccasins" are not hypothetical—they are in existence; a full series of "moccasin-boots," illustrating the transformation of the moccasin into a boot, is given by me (*cf.* fig. 24, 59 and 71–84 in *Moccasins*). The question remains, of course, *how large* a share the moccasin has had in the evolution of the footwear of northern Asia. When the transformed moccasin at last loses all its moccasin-traits, we lose track of it. I have never asserted that *all* boots of northern Asia were moccasin-boots—on the contrary, I have proven that there are boot-forms in northern Asia which have nothing to do with the moccasin, but are evolved directly from the sandal and the stocking.

Dr. Laufer regrets that I have not cited the *Publications of the Jesup North Pacific Expedition*, Leopold v. Schrenck's work on the Amur peoples, nor the Russian literature. The reason for this is quite simple. I did use the well-known publications of the Jesup expedition and of v. Schrenck in my book on arctic clothing, and also the Russian literature, as far as it was available, was used and cited by me in that work. I have not, however, found anything in these very valuable publications that was of any use to me in my moccasin paper, therefore I did not cite them there. Of course, they contain descriptions and illustrations of footwear, but no information about moccasins or "transformed moccasins" which was not to be had in fuller and better detail by studying the collections in museums. I have not found it expedient to cite unnecessarily all the authors that have described moccasin-forms; only when I have learned something from them on this particular subject, have I mentioned their publications. Nor have I cited the immense number of more or less superficial and occasional descriptions of American moccasins, which I have come across in my search of information.

On the last two pages of my moccasin paper, I have ventured to set forth a very far-reaching hypothesis which—it may be justly said—is as much out of proportion to the study of moccasins as a church-steeple to a hut. I was much in doubt whether I should not abstain from such short and extremely insufficient mentioning of my "inland and coastal culture" theory; it was beyond my power, however, to suppress altogether my "working hypothesis," the underlying idea which has made even boots and old pelts interesting to me. By tracing my hypothesis in such short outline, I have, however, invited misunderstanding. My theory is not, as Dr. Laufer thinks, based upon the geographical dis-

tribution of one single ethnographical phenomenon—it is supported not only by the data of clothing and of skin-dressing, but also (as suggested on p. 249 of my moccasin paper) by the distribution of different types of habitation, means of transportation, modes of hunting, reindeer-nomadism, etc. In fact, the theory rests upon the geographical distribution of the types of material culture in the northern regions, and would require for its elucidation a full and detailed analysis of all the different sides of the material culture of northern tribes. The preparation of such a full presentment of my theory demands, however, more work than can be done in a short while.

When I find the "inland culture" fullest developed and least mixed with "coastal culture" among the Tungusians of Siberia, I do not intend to say that the Tungusians were the first originators of the "inland culture." Nor do I hold the opinion that the oldest "inland culture" was identical with the Tungusian culture of the present day. The Tungusian area contains the highest development of the "inland culture," whose older aspects are represented outside of this area; for this reason I hold the opinion that the geographic area, now inhabited by the Tungusians, has been from the outset of great importance for the development of "inland culture." A chief cause of this probably was an influence of cultural stimulants from the south.[1] On the whole, it is not among the Tungusians that we must look for the oldest traits of the "inland culture"—the evolution of "inland culture" has been most rapid there and has effaced the old traits. For this very reason, however, I find it probable that the area, now inhabited by Tungusians, contained the fountain-head of the "inland culture," perhaps long before the Tungusians entered Siberia.

I do not at all presume "that the ethnical conditions of northern Asia were the same anciently as at present." Such an assumption would involve on my part a gross and unpardonable ignorance of ethnological literature. I find it, however, extremely useful to study the geographic

[1] As mentioned on p. 249 of *Moccasins, etc.*, I find in reindeer-nomadism certain elements due to influence from more southern forms of nomadism. These southern elements are nowhere as apparent as among the Tungusian reindeer-nomads. The use of the reindeer for riding certainly represents a loan from the nomadism of Central Asia and probably is younger than the use of the reindeer as a draught-animal; the types of harness used in driving with reindeer are variations of the same fundamental type everywhere from the Lapp to the Chuckchee (this "barbaric" spelling I have adopted from the *Publications of the Jesup Expedition*), and there is a very significant similarity between this type and the forms of dog-harness used by the Gilyak and by the old Kamchadal. The methods employed in herding the reindeer, however, contain some elements which have their root in the reindeer-hunt.

distribution of ethnographical phenomena, not only on the American continent, but on the Asiatic as well. Historic sources should, of course, be consulted whenever possible; they are, however, never sufficient when we want to reconstruct the culture of bygone ages—not in Europe, still less in northern Asia. As to moccasins and snowshoes, a theory about their origin and spread must always be founded upon a study of the actual forms and their geographical distribution; this is what I have tried to do. I have not attempted to "reconstruct a history" of the peoples of northern Asia.

I am quite aware that my assumption of a Siberian origin of the Lappish footwear is somewhat strengthened by the fact that a considerable part of the Lapp culture is closely related not only to that of the Samoyed, but also to that of several other Siberian tribes. It is hardly possible to study the cultures of northern Europe and Asia for years without recognizing this fact. It would, however, have been ridiculously out of place, if I had drawn the question of the origin and history of the entire Lapp culture into the discussion of moccasin-forms—this question is so intricate and many-sided, that it would require very considerable space.

GUDMUND HATT

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Dr. Laufer has called attention to some inconsistencies and infelicities in the spelling of the proper names in Dr. Hatt's paper. The editor is solely responsible for these, for when a choice of spelling is permitted the editor makes the choice that there may be a certain amount of uniformity and consistency running through the various volumes. The retention of F. C. M. to refer to the institution now rechristened Field Museum of Natural History is also the fault of the editor, who hesitated to make the change when some of the very numerous occurrences of the abbreviation in the manuscript were almost sure to be overlooked.—Ed.

THE TWO BELIEFS IN PERSONAL IMMORTALITY

THE scientific writer on religious subjects is often discussed by incompetent or prejudiced persons. When, as in the case of the review of my book[1] by Professor Ogburn in the last issue of this journal, the reviewer is both competent and dispassionate the writer may well think himself fortunate.

Professor Ogburn wishes that I had published more fully the tables upon which my statistical conclusions are based. It was my desire to do so, but after bearing the expense of the investigation itself, I shrank

[1] *The Belief in God and in Immortality. A Psychological, Anthropological, and Statistical Study.* Sherman, French and Co.: Boston, 1916. Pp. XVII + 340.

from increasing the printer's bill by the publication of tables that after all were not necessary.

I cannot quite accept the criticism referring to Part I (the Two Conceptions of Immortality, their Origins, etc.). Professor Ogburn accepts in general the distinction I established between the primary and the modern conceptions of immortality, but it seems to him that I "neglect the importance of desire among primitive peoples," and that I magnify the difference between the primary idea and those of the moderns." Stripped of all non-essentials, what I set forth was that certain facts to which the savage ascribes external validity, such as visions in sleep, fever, etc., convince him of survival after death; and that a belief arising in that way, *i. e.*, based on what is considered by the savage as actual perception is, in principle, independent of desire. That belief I have called "primary." Whether the savage likes it or not, ghosts are there; he sees, hears, and even touches them. The belief is *imposed upon him* just as that in the existence of the natural objects with which he is surrounded. Such a belief has no moral significance in the sense of reward or punishment for conduct on earth.

Quite different in origin, nature, and function is the modern belief. It originated not in the class of facts which produced the primary belief, but in desire for the realization of ideals of happiness and self-realization. A conception of immortality arising in this manner could not fail to possess the characteristics desired by man and, thus, to become a source of encouragement and inspiration.

Now, it is a matter of record that in many tribes the conception of continuation after death bears little or no trace of having been elaborated into a paradise under the influence of desires imperfectly gratified in this life. These tribes hold the primary belief in its pure form. Its characteristics are adequately accounted for when the origin I have ascribed to the primary belief is admitted. I have not failed to recognize, however, that in other tribes the conception of immortality bears marks of greater complexity than that belonging to the primary belief: it assumes the aspect of a paradise. In the first chapter of my book, under the subtitle "The Primary Paradise" (pp. 15–23), I described, briefly it is true but quite definitely, the presence among savages of desires acting upon the idea of continuation after death. Here we find operating the two classes of factors: perceptual experiences and desires.

But when one passes from these tribes to the higher social level occupied by the peoples bordering the eastern end of the Mediterranean sea, those from which Europe derived its civilization, one finds at the

beginning of the historical period a conception of continuation after death altogether free from the influence of desire. Survival was accepted by those peoples as an unavoidable fact, undesirable, and often to be dreaded. Perhaps my reviewer has not sufficiently considered the demonstration made in chapter III, that among the peoples I have designated the conception of the hereafter was of an undesirable existence to which all were doomed and from which the idea of retribution was as completely absent as that of the gratification of any craving for happiness and self-realization. It is of that belief that I speak when I say that it is radically different from the modern one in point of origin, nature, and function; and that the modern belief is not a continuation of the old but a new creation. The facts are, it seems to me, incontestable; some of them I have related in chapter IV.

The paradisiacal elements that appear in the conception of the hereafter entertained among certain savage tribes, does not disprove the theory of discontinuity between the primary and the modern conception, for these elements had disappeared when the modern belief came into existence.

Why did these paradisiacal elements, born of desire, disappear? Why did not desires assert themselves more and more effectively and gradually transform the primary conception into the modern one? This is a very interesting question toward the solution of which I offered the following suggestion:

> The explanation of the temporary triumph of the dismal belief in impotent and vacuous ghosts seem to be found, as I have already intimated, in the inability of men at that stage of culture to conceive of a person as enjoying a tolerable existence when deprived of his earthly body. The persistence of the difficulty offered by the destruction of the body is sufficiently evidenced by the fact that its resurrection is affirmed in the modern conception of immortality (pp. 97–98).

I might add that I do not regard my study of the origin of the beliefs in immortality as complete. My task was a narrowly circumscribed one. The student of social psychology will want to know what are the conditions for the coming into existence of the desires to which I referred the origin of modern immortality. Why did they appear at a particular time, among particular peoples, with an intensity sufficient to break through all impediments to belief? In answer to these questions nothing more than hints are provided in my book.

The problem of origin as I have treated it, comes much nearer being fully solved in the case of the primary belief. For, given a certain

intellectual level and apparitions of deceased persons in dreams and other visions, and the belief seems bound to appear.

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THE DIFFUSION OF CLANS IN NORTH AMERICA

IN a recent article on "Iroquoian Clans and Phratries" (*American Anthropologist*, vol. 19, 1917, pp. 392–402), Mr. C. M. Barbeau advances certain views with reference to the dissemination of clans in North America which to many will appear as both interesting and startling. The keynote of Mr. Barbeau's position is struck in the following paragraph:

> The Iroquoian and neighboring Algonkian clans, on the whole, were widely diffused exogamic kinship units, endowed with inconspicuous crest and supernatural guardian systems, the membership of which was inherited in the maternal line or conferred through adoption. As will be seen in the following lists, the clans were few in number; but their component parts were found disseminated in sections of numerous towns of many tribes and nations. Distance and linguistic barriers did not prevent the members of the same clan from acknowledging their relationship and mutually abiding by the rules of exogamy and fraternal help (p. 393).

Having enumerated the clans of the different Iroquoian tribes and indicated their division into phratries, the author remarks:

> In the various Algonkian, Siouan, Muskhogean, Yuchi, Pueblo, and other lists of clans we find, unevenly represented, most of the Iroquoian clans, besides many others. *The diffusion of the different clans here is in all probability due to direct transmission or to imitation*[1] (p. 395).

Having thus conceived the distribution of clans of the same name as due to "diffusion," the author proceeds to indicate the distribution of the various "clans" among the tribes of North America.

The novel method of identifying historically clans of the same name is the more striking, as the author does not reveal any consciousness on his part of any special problem involved, but seems to regard the interpretation through diffusion as vindicated by the mere facts of distribution. The author takes a similar view regarding the distribution of phratries, expressing his conclusion in the following words:

> There is little doubt that, in the course of time, the moiety or phratric system as well as specific eponymous clans spread independently in various directions, from as many unique, but not necessarily identical, centers (p. 398).

[1] The italics are mine.

I must characterize the author's procedure as highly artificial; in fact, if consistently carried out, it would lead to quite impossible situations.

In every problem of diffusion *vs.* independent development—and our case certainly presents such a problem—the first task of the ethnologist is to analyze the similarities of the cultural traits or features in question, for on his insight into the nature and degree of these similarities will depend his initial view of the two alternative historical explanations. What, then, is the similarity on which Mr. Barbeau bases his verdict of diffusion? It is not the similarity of the social unit as such, the clan or gens, nor is it the similarity of the clan concepts of two or more tribes; it is solely the similarity of names. Two or more cultural features—in this case clans—are identified on account of sameness of name and, on the basis of the identity of this one trait, the name, are regarded as historically related by means of the process of diffusion. So far reaching a conclusion based on apparently so slight a trait would lead one to expect that the names thus found identical are most unusual or peculiar, or in some other way unique, thus precluding the possibility of their multiple independent origin. Further inspection, however, reveals no such peculiarities. Names of common American animals—Bear, Deer, Turtle—are given to clans, just as the names of animals indigenous in other countries are given to the clans which occur in such countries. Many of the animals thus used as clan names in North America occur over wide geographical areas, and so it comes to be, as might be anticipated, that several of the animals appear as clan names in different tribes. The situation is as it should be; thus there is no occasion to advance special explanations, such as diffusion, to account for the phenomena, unless other reasons may be advanced for the procedure in specific cases. Thus many of the Iroquois clans, no doubt, had a common historical origin; and the same may be said about several gentes in the Southern Siouan area, and a large number of clans in the Pueblo area. In these and other similar instances clans of identical name were doubtless often the offshoots of one original clan, or the sameness of the name was due to imitation. But the theory that the common North American clan names must be assigned single origins and specific distributions by diffusion, is as phantastic as it is unnecessary.

The same reasoning applies, of course, to the phratries. It seems especially peculiar that Mr. Barbeau, having successfully demonstrated the fundamental differences between the phratries of the Iroquois and those of the Northwest coast, should, nevertheless, feel "that the various

phratric and moiety systems, appearing sporadically in many parts of North America, may have had a common remote origin or a single center of diffusion" (*American Anthropologist*, vol. 19, 1917, p. 405). In situations such as this a wider comparative outlook will often prove of value. If phratries in the form of dual divisions associated with clans or gentes were an exceptional phenomenon, restricted in its distribution, say, to North America, this would constitute a *prima facie* justification of an attempt to correlate historically the several moiety systems of that continent. But such is far from being the case. Moieties in association with clans are all but universal in Australia and very common in Melanesia; hence one is not surprised to encounter them in some of the clan (or gentile) areas of North America, and a check is put on overzealous attempts to apply the principle of diffusion.

I must confess to a sense of keen disappointment that an American student of the thoroughness and critical acumen of Mr. Barbeau should have shown in a field with which his familiarity cannot be doubted so little grasp of the fundamental methodological principles involved in problems of diffusion and independent development.

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THE CONSTITUTION OF THE FIVE NATIONS: A REPLY

STUDENTS of Iroquoian social and political organization and folklore are fortunate in having so able a source of data as Mr. J. N. B. Hewitt. In the *Anthropologist*, vol. 19, no. 3, Mr. Hewitt criticized one of my recent publications, *The Constitution of the Five Nations*, and most ably pointed out both the faults of the native authorities who supplied my information and the errors in editing. In an earlier issue of the *Anthropologist* Dr. A. A. Goldenweiser published a criticism. In justice to the subject it would have been well and saved a possible misunderstanding if both critics had read page 12 and 13 of the introduction. There I said:

> The two principal manuscripts that form the basis of this work were found on the Six Nations Reservation, Ontario, Canada, in 1910.
>
> The first manuscript was a lengthy account of the Dekanawida legend and an account of the Confederate Iroquois laws. This material has been brought together by Seth Newhouse, a Mohawk who has expended a large amount of time and given the subject a lengthy study. His account written in Indian English was submitted to Albert Cusick, a New York Tuscarora-Onondaga, for review and criticism. Mr. Cusick had long been an authority on Iroquois law

and civic rites, and had been a chief informant for Horatio Hale, William Beauchamp, and in several instances for the present writer. Mr. Cusick was employed for more than a month in correcting the Newhouse manuscript until he believed the form in which it is now presented fairly correct and at least as accurate as a free translation could be made. The second manuscript was compiled by the chiefs of the Six Nations Council and in the form here published has been reviewed and corrected by several of their own number including Chiefs John Gibson, Jacob Johnson and John William Elliott. The official copy was made by Hilton Hill, a Seneca, then employed by the Dominion Superintendent for the Six Nations. It has been reviewed and changes were suggested by Albert Cusick. . . .

In presenting these documents the original orthography has been retained. The only attempt to record Iroquois names and words phonetically is in the notes. This will account for some variations in spelling. . . .

In the light of the conditions under which the Bulletin under discussion was presented, a compilation of native documents, criticism seems gratuitous. Especially significant is Mr. Hewitt's attempt to controvert my statement of Mr. Cusick's help. One would almost suspect this to be designed to impute a falsehood, but in the light of Mr. Cusick's assistance, this imputation would seem to fall little short of maliciousness though probably not so intended.

The reference to "a free translation" should be apparent to anyone who has read the work under discussion. Suffice to say, no translation or presentation in English can gracefully and fluently express the Iroquoian idiom. Witness Mr. Hewitt's own literal translation of the "Iroquois Cosmology." It appears in clumsy, stilted English, involved and lacking in force of expression. Literal translation robs the native thought of much of its meaning and emphasis.

Our critic's reference to wampum would seem to imply that only one sort of wampum was recognized by us, though the manuscripts clearly name elderberry twigs, scouring rushes and porcupine quills. The wampum belts described as "constitution belts" may be regarded as such even though not made during the days of Dekanawida, in this sense being as truly memorials to the founding of the League as Lincoln's Gettysburg speech is Lincoln's still even though printed in today's newspaper. The belts are old and probably native made and they have been invested with the symbolism ascribed to them—memorials of the days when Dekanawida spoke.

The lack of accuracy, consistency and forethought on the part of the authors of the manuscripts is to be deplored but even though these Indian annalists wrote clumsily it did not occur to me that of my own

initiative I should alter their writings, even for the sake of presenting them as I personally desired to see them. Mr. Hewitt must learn that if ethnologists should habitually change the myths and native manuscripts that came into their hands, in order to bring about consistency, the finished production would shrink in value. The scientist takes what comes to him from the quarry, and though it is covered by corrosion and foreign matter, he presents it as found. It is his specimen upon which he does not chisel an inscription. That is written on a separate label.

An example of native inaccuracy is quoted by Mr. Hewitt in the following: "After a journey across the lake (Ontario) he came into the hunting territory of the Flint Nation." Mr. Hewitt correctly stated that the immediate landing place of Dekanawidah would be in Oneida territory. Our Indian writer simply described things rapidly and without detailed chronological sequence, yet if some other writer had penned a line such as "After a journey across the ocean Olaf Jensen came into the forests of Minnesota," we think few critics would have deliberately gone out of their way to say that the assertion implied that Minnesota was on the Atlantic coast, especially if the statement had been made to those familiar with geography.

We accept in a proper spirit the catalogue of our own blunders but we must insist that we do not believe that in presenting the Indian manuscripts, we should eliminate their "crudity and naïvete from consideration," even to satisfy those who possess other versions of these Iroquois codes and legends. Indians who were life-long residents of their respective reservations produced the documents and stood for them. The writings represent in English, so far as they were able to make them, what they thought, believed, and lived in Iroquois. They do not necessarily represent what the present writer thinks accurate in detail or satisfactory.

Mr. Hewitt has had a large influence in directing the minds of his informants and no doubt, as he himself suggests, has contributed largely to their store of ancient lore, though we must confess it seems to us that "facts" so collected seem like re-importations; in other words, like telling one's informants what to say and how to say it. For example, Mr. Hewitt tells in his criticism how he instructed Mr. Newhouse in a certain translation of Lafitau, and says that Newhouse accepted the data and incorporated it in his code, Section 93–96. Mr. Hewitt also tells how he instructed the chiefs in the translation of certain names. It is thus evident that my distinguished critic has had an enormous advantage in previously instructing for a period of years his native informants.

They have accepted his statements as correct and incorporated them in their writings as original with them. The extent of this may be realized when it is said that some of the chiefs admitted that Mr. Hewitt wrote the introduction to the chief's version.

In our translation of the "record" staff, a cut of which was published, we simply followed the translation made by Abram Charles, a chief of the Cayugas, for Mrs. H. M. Converse, at least twenty years ago. Mr. Cusick apparently was satisfied with the translation. However, we suspected that it might be an attempt to call to mind the so-called condolence ceremony and thus we placed the picture to face that text, and with considerable difficulty, but Mr. Hewitt evidently thinks it a coincidence.

We are grateful to Mr. Hewitt's criticism, for he has pointed out a store of facts that should have been made available years ago. Modestly he refrains from more extensive criticism, but we hope to have all the necessary data when he publishes his own version of "The Constitution of the League" for which he has prepared native texts in Mohawk and Onondaga. An English parallel in Mr. Hewitt's own fluent English will then be available and, of course, be above criticism, though there will be some who will suspect that the content and the "original text" have been rigidly supervised.

Apparently Mr. Hewitt agrees with Dr. Goldenweiser's earlier criticism, and yet Dr. Goldenweiser specifically states that "The Constitution of the Five Nations is a figment. . . . It does not exist . . . either written or unwritten." Strangely, however, it appears as a coincident that Mr. Hewitt's texts and translations parallel those we have published, for the *Twenty-eighth Report of the Bureau of Ethnology* mentions his collection of "texts" in the Onondaga and Mohawk dialects,

> embodying the basic principles of the civil and political structures and organization of the League of the Iroquois and data relating thereto. The following captions will indicate sufficiently the subject matter of these texts: The Constitution of the League, the Powers of Thadodaho, Amendments, Powers and Rights of Chiefs, Powers and Rights of Women, Powers of the Women Chiefs, etc.

We confess that we do not quite understand Mr. Hewitt's concluding statement,

> I have purposely not given out this unfavorable estimate of Mr. Parker's recent work until it had been reviewed by one whose motives Mr. Parker might not question.

The pure love of accuracy is sufficient motive, and should have prevented

any feeling of restraint in giving out "this unfavorable estimate," until some other ethnologist has taken the initatii e. We trust that this inertia of Mr. Hewitt will now be overcome and that we may be prevented from getting into further sloughs of error by his speedy publication of his own version of the "Constitution of the Five Nations." We feel sure that the faults of our own attempt will but add to the luster of the greater work that is to come.

Like Kipling's hero in *The Neolithic Age*, I feel, as I survey the bulky criticism of my bulletin, as if ". . . a rival of Solutré told the tribe my style was *outré*. . . ." But I am consoled, as every ethnologist must be who finds dozens of versions of myths and "constitutions," in the last verse of the poem, and for a pleasant thought, I present it to my critics.

Here is wisdom for your use, as I learned it when the moose
And reindeer roared where Paris roars tonight
There are nine and sixty ways of constructing tribal lays,
And—every—single—one—of—them—is—right.

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CULTURE CONTACT AND MIGRATION *versus* INDEPENDENT ORIGIN: A PLEA FOR MORE LIGHT

IN his review in this journal[1] of a volume on Oceanic mythology of which I am guilty, and more especially in a more recent discussion of it published elsewhere,[2] Dr. Lowie has taken a stand on the question of culture contact and migration *versus* independent origin that seems to call for a few words of explanation on his part. The matter at issue is one of such general importance and interest, and Dr. Lowie's most recent statements are so puzzling, that it would seem in order for him to bear witness somewhat more fully, to the faith that is in him.

Somewhat hesitatingly in this journal, but with complete assurance in his latest review, Dr. Lowie declares that explanations of cultural similarities and differences as due in any measure to migrations (or even culture contact!) are woefully out of date—he suggests indeed, that no sane person nowadays even condescends to consider such a discarded and worn out hypothesis, which is after all but a "curious disease" which has infected ethnological thinking during the last decade. For those who will persist in such puerile explanations, it is clear that Dr.

[1] *American Anthropologist* (N. S.), vol. 19, pp. 86–88.

[2] *The New Republic*, vol. XIII, no. 166 (Jan. 5, 1918), pp. 288–289.

Lowie feels only pity. Now everyone knows that a reviewer must be magisterial, but I must confess to a certain amusement at finding one so loftily denouncing, when used by others, an explanation he so constantly employs with telling effect himself; and who, after gravely rebuking an unfortunate author for "contradiction and confusion," himself sins egregiously in this very fashion a few sentences further on!

During the last few years Dr. Lowie has studied with great success the social organization of the Plains tribes. With rare skill he has analyzed the complexities of the military and other societies, has compared their forms in different tribes, and in a final summary paper, admirably drawn his conclusions as to the origin, growth, and spread of these interesting institutions. In all of this work he has constantly employed these very principles of culture contact and the influence of migration, which he so loudly decries when applied by another to Oceanic problems. On almost every page of his final summary, existing conditions are explained as due to borrowing or adoption consequent on direct or indirect contact, and these contacts, relied upon to explain the facts, are frequently substantiated and vouched for by reference to known or hypothetical tribal movements. On such evidence, amply confirmed by a mass of data on the material culture, mythology, language, etc., we have come to regard the Plains area as one in which, in addition to local developments, there has been extensive mingling of cultural elements, in part at least due to migrations. Now this is precisely the case in Oceania. A vast mass of evidence exists to show that the peoples of this whole region are, to a far greater extent than in the Plains, racially complex, and that the existing cultural conditions are the result of widespread borrowings and popular movements. To brand, therefore, as absurd, an hypothesis that the two sharply contrasted types of cosmogony in Polynesia are the result, in large part, of the mingling and interaction of two successive migratory waves, is, to say the least, amazing—the more so in that the type attributed to one of these waves, shows such demonstrable relationship to the form still retained by those members of the group yet remaining in their earlier western home. In view, moreover, of the critic's constant employment of the same hypothesis in his own field, his position seems quite illogical.

Dr. Lowie's denial of the necessity or even the value of any consideration of culture contacts or migrations in dealing with mythological problems in the Polynesian area is the more perplexing in view of his statements in the very next paragraph. In this he declares that the author has committed the unpardonable sin—he did not tabulate all

similarities and analogies between Oceanic and American mythologies, to the end that some light might be thrown on the "question whether New World culture was deeply or at all affected by contact with alien civilization"! In other words, Dr. Lowie recognizes the possible influence in America (of necessity as the result of culture contacts or migrations) of Oceanic cultures, which would thus explain certain well known and puzzling resemblances between the two areas. He thus admits in this instance the validity of the explanation which he denies in the Oceanic area where it is, on general principles, far more probable. In one breath thus the author is soundly berated for daring to suggest, that a complex mythology may owe much of its complexity to a blending of elements as a result of direct or indirect contacts brought about by migration, in an area moreover, where many such contacts and movements are proven; in the next he is just as strongly condemned for *not* discussing at length and furnishing data upon similar supposed contacts and migrations, where none have ever been shown! May we not ask in the interests of clear thinking, that Dr. Lowie decide whether he will run with the hare or cry with the hounds! To try to do both at once usually leads to disaster.

The reviewer offers, however, not only destructive but constructive criticism. Having discarded as beneath contempt the theory of a dual origin for Polynesian cosmogonic myths, he vouchsafes to reveal to us only true faith. He accepts without reservation the conclusion that the evolutionary type is, in Polynesia, more ancient than the "creationist" form—indeed questions the sanity of anyone who should think otherwise—and then accounts for the origin of the former type, by declaring it to be a "superstructure of fine-spun metaphysical abstractions" built by "a priestly caste" on "the foundation furnished by the mythology of the laity." Now if, as he states, "intra-tribal causes" are amply sufficient to account for all the phenomena, and there is "not the slightest reason" for "dragging in" any extra-Polynesian influences, then the two admittedly different types of cosmogony must be the result of a differentiation in an isolated community, of an originally single type—the one must be the surviving beliefs of the "laity," the other the airy structure reared upon this by the priests. But alas! we are told that no "sane" person can doubt but that the metaphysical evolutionary type *preceded* in point of time, the "creationist" form—*on* which we are now told it was erected and *out of* which it grew! I must confess that I cannot quite follow here Dr. Lowie's dialectic, and cannot but have a suspicion that such "pitfalls" of "contradiction and confusion" might

have been "avoided by a very simple device, which might be heeded by ethnologists generally"—that of looking where you are going!

At this point, however, I am suddenly overcome by remorse—perhaps I have done Dr. Lowie a grave injustice and have quite falsely accused him of "contradiction and confusion." Perhaps we are talking about different things. When he speaks of a "creationist" type, he may mean, not what I have spoken of as the "creative" form, but rather that old Maori conception in which for the greater gods, no ancestry at all is given; although why the term "creationist" should be used for this is not clear. If that is the case, and *that* is what he means by "creationist," then I might agree with him that there is no *necessity* for bringing in any dual element, and that the evolutionary metaphysical type *may* be in New Zealand a local outgrowth of this—as I have indeed suggested! (p. 10). But even if this *is* what he means by the "creationist" type, a moment's reflection will make it clear that the true faith still requires of its believers unquestioning acceptance of the theory that the result precedes the cause, the child is father of the parent, and that the builder should first rear his airy spires and then proceed to put the foundations under them! Further, if by "creationist" the old Maori form is meant, Dr. Lowie directly misrepresents me, for far from saying that the evolutionary type antedates this, on the page mentioned *this* "creationist" type is referred to as "a survival of the older New Zealand belief," and the evolutionary form is contrasted with it as a "later development." Just what Dr. Lowie's meaning is in this whole matter seems doubtful, and he would appear to be in a dilemma, for either he has trapped himself in a contradiction, or he has read his text carelessly and unwittingly misrepresented his author's views. Can he suggest a *tertium quid*?

Apart, however, from all unimportant and petty detail, there is here a real question of far-reaching importance. If Dr. Lowie is convinced that the two types of cosmogonic myths in Polynesia as I have defined them, owe their divergence *in no degree whatsoever* to the interaction of two or more different currents of thought, brought into Polynesia directly and indirectly as a result of migrations, but have developed in isolation out of a single original type through the working only of "speculative fancy," I am sure there are others beside myself who would welcome a somewhat more specific and detailed exposition of his theory. This must, of course, if it is to supplant the other hypothesis, explain not only the peculiar distribution of these two special features within the Polynesian and Oceanic areas, but must also account, at least as well as the other theory, for the observed characteristics and

distribution of the other types of myths. For, if the varied forms of cosmogonic tales are purely of local "intra-tribal" growth, why should there be any need to invoke other than "intra-tribal causes" to account for differences observed in other elements of the mythology? Or, for that matter, in *any* aspect of Polynesian culture? The apparent consequences of Dr. Lowie's theory are thus seen to be really momentous, for it necessitates the assumption that the Polynesian area has been for a long period that rarest of anthropological regions—a veritable *mare clausum;* an isolated area closed to all outside influences and developing its culture in complete seclusion. It must also be noted, that explicitly for New Zealand, and implicitly for the whole of Polynesia, Dr. Lowie further assumes a "homogeneous population"; indeed he could hardly do otherwise in view of his denial of all migrations. If his postulate is true, it has, of course, a very important bearing upon the whole question. Inasmuch, however, as this assumption of homogeneity directly contradicts currently accepted opinions, together with the mass of accessible facts, Dr. Lowie must have at his disposal new and very important data to warrant him in his conclusions, and it is certainly incumbent upon him to produce these for the benefit of other students.

R. B. DIXON

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ANTHROPOLOGICAL NOTES

MR. FREDERICK WEBB HODGE, who served as editor of the *American Anthropologist* from its foundation until 1915, except for a brief interval, has resigned from the Bureau of American Ethnology, Washington, D. C. Mr. Hodge became connected with the Bureau in 1889 and since 1910 has directed its work as Ethnologist-in-Charge. Among the numerous tasks of importance may be mentioned the supervision and editing of Bulletin 30, "The Handbook of American Indians." Mr. Hodge has accepted a position with The Museum of the American Indian, Heye Foundation, of New York, which will permit him to engage personally in archaeological field work in the Southwest and elsewhere. This opportunity promises well for our science as well as for Mr. Hodge who began his anthropological work in the Southwest as a member of the Hemingway Expedition.

DR. JESSE WALTER FEWKES has been appointed Chief of the Bureau of American Ethnology, relieving Mr. Hodge, whose resignation took effect February 28.

Dr. Fewkes' archaeological and ethnological activities are well known. He was the first to develop a continued interest in the Hopi myths and ceremonies. As a natural result of this interest he has been engaged in archaeological excavation and exploration which has taken him well over the Southwest area. His work has centered however in the vicinity of the Hopi and in the Little Colorado. He has been instrumental in the restoration and preservation of several of the most important of the community house ruins.

This position of responsibility and honor is a fitting reward for the one surviving founder of modern anthropological research in America.

IN honor of the appointment of Dr. J. W. Fewkes as Chief of the Bureau of American Ethnology, a complimentary luncheon was tendered him and Mrs. Fewkes at the Smithsonian Institution on Friday, March first. Every member of the staff and all the employees of the Bureau were present. At the close of the luncheon Dr. Fewkes made a brief address, recalling the high traditions of the Bureau and outlining plans for its further development. The primary objects of its ethnologic research were defined by Dr. Fewkes as "Man in America,—where did he come

from, how long has he been here, and what has he been doing since he came?" Short speeches were made by members of the Bureau, the first speaker being Mr. James Mooney, who noted that the study of ethnology tends to bind the whole human race together by securing a better understanding of mankind.

MISS FRANCES DENSMORE, of the Bureau of American Ethnology, returned to Washington in November from field work in Minnesota.

THE establishment of a new anthropological journal has been announced. This journal is to be known as the *American Journal of Physical Anthropology*. Its founder is Dr. Aleš Hrdlička who is also the editor-in-chief. The appearance of this new *Journal of Physical Anthropology* promises to be an important event in the history of anthropology in America. There was a time when the *American Anthropologist* as the organ of the American Anthropological Association could give adequate space for the discussion of all phases of anthropological research, but the development of anthropology in America has reached a stage where no single journal can meet the needs of all investigators. The movement for differentiation began with the establishment of the *International Journal of American Linguistics* in 1917 and may be said to be consummated by the establishment of the present *Journal of Physical Anthropology*. The *American Anthropologist* is still the general organ of anthropology as a whole and will give space to general theoretical and coordinating discussions. Also, it will continue for the present, at least, to be the medium for the presentation of cultural problems, both historic and prehistoric.

The new *Journal of Physical Anthropology* will give particular attention to the anthropological problems contingent upon the war and will, for the time being, be active in supporting the work of the Committee on Anthropology of the National Research Council. The editor has associated with himself a highly representative board of editors, as follows:

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Dr. Aleš Hrdlička, who is the leading physical anthropologist in the United States, is to be congratulated upon the great service he has rendered to American anthropology and we are sure that he will receive the earnest support of all members of the Association.

DR. HERMAN K. HAEBERLIN, Assistant in the Department of Anthropology of Columbia University and the American Museum of Natural History, died at Cambridge, Mass., February 12, 1918. A later issue of the *Anthropologist* will contain an appreciation of Dr. Haeberlin's all too brief anthropological activities.

AT the meetings of the Anthropological Society of Philadelphia Dr. A. A. Goldenweiser in January presented a paper on "The Problem of Culture Diffusion" and in February Dr. Boas spoke on "Thought as Determined by Social Classes."

PROFESSOR GEORGE GRANT MACCURDY, of Yale University, has been made a member of the Committee on Anthropology of the National Research Council.

YALE UNIVERSITY has been left the sum of $50,000 under the will of Evelyn MacCurdy Salisbury, widow of the late Professor Edward E. Salisbury, to found a professorship of anthropology. The chair, which by the conditions of the will is to be occupied by George Grant MacCurdy, is to be known as the Charles J. MacCurdy Professorship of Anthropology. Additional provision is made for the maintenance of the Department and a further sum of $60,000 is to be available in the future at the discretion of Professor MacCurdy.

MR. HOMER E. SARGENT has recently presented to the Field Museum of Natural History, Chicago, his remarkable collection of old Southwest textiles, chiefly Hopi and Navajo blankets, many of them bayetas. This

gift represents the result of judicious and discriminative collecting extended over a period of more than ten years. and includes a great deal of material that at the present time would be no longer obtainable.

DR. PAUL RADIN, Professor of Anthropology at Mills College, delivered a series of five lectures at the University of California on "The Relations of History and Ethnology." Dr. Radin's courses at Mills College have been devoted to general anthropology, North American ethnography and primitive literature.

MR. JACOB T. BOWNE, librarian of the Young Men's Christian Association College, has presented his anthropological collection to the Springfield Museum of Natural History. The collection consists of some thousands of objects collected in the Connecticut valley near Springfield. In addition to the specimens, the gift includes several hundred books on anthropology, together with archaeological cabinets, manuscripts, maps and diagrams.

DR. V. GIUFFRIDA-RUGGERI, Professor of anthropology in the University of Naples, was made on November 29, 1917, corresponding member of the École d'Anthropologie. On December 11, he was elected an honorary member of the Royal Anthropological Institute of Great Britain and Ireland.

IN October of last year the Hon. George Washington Grayson, of Eufaula, Okla., well known to students of the ethnology of the five civilized tribes for his interest in everything concerning the past history and present well-being of his people, was appointed by President Wilson to succeed Moty Tiger as Chief of the Creek Nation.

THOSE having copies of volume XVII, nos. 1 and 2, and volume XVI, no. 3, in good condition, may receive $1.00 per copy for them by addressing the Secretary or Editor.

NEIL M. JUDD, Treasurer of the Association, joined the Signal Reserve Corps as a volunteer, early in the year. Dr. William C. Farabee of the University Museum, Philadelphia, Acting Secretary of the Association, has taken over temporarily the duties of Treasurer.

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